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PARALLELISM IN DEVELOPMENT.

By R. LYDEKKER, B.A.Cantab.

IN the course of our three preceding articles in KNOWLEDGE it has been to a great extent our aim to show that certain animals may resemble one another very closely in general external appearance, or may possess certain peculiar structural features in common, without being in any way intimately related; thus rendering it evident that such similarities of form or structure have been independently acquired, and are not inherited from a common ancestor. It has been shown, for instance, that mammals belonging to several distinct orders, or to different families of the same order, may assume such a marked external resemblance to the common mole as to be designated in popular language by the same general title; while in other cases a more or less striking approximation to the type of the ordinary hedgehog has resulted from the independent development of very similar spines in totally distinct groups. Furthermore, it has been pointed out in the third of the three articles that large tusks of very similar form may be independently developed in the jaws of totally different distinct groups of mammals; and even that certain extinct reptiles have acquired tusks which are almost indistinguishable from those of some of the carnivorous mammals, there being no direct relationship between the members of these groups.

With regard to the external similarities of form in the above-mentioned instances, we have seen reason to believe that their inducing cause has been either similarity of habit or the need of protection; while in the case of the tusks the similarity may in certain instances be due to the necessity for efficient offensive weapons. Be their causes, however, what they may, it is evident that such resemblances among animals, which are, so to speak, accidental, indicate what may be termed a kind of parallel development; and as such parallelism in development, or shortly "parallelism," has recently attracted a good deal of attention among biologists, we propose in this communication to present to our readers some of the more striking instances of such similarity. In the instances above alluded to, the parallelism is either to a great extent shown in external characters, or in structures which are easily modified; but, as we shall indicate in the sequel, in other cases it affects deeply-seated structures; and its inducing cause is then very difficult to surmise on any of the ordinarily accepted doctrines of evolution. It will, moreover, be obvious that the acceptation of parallelism in development—and accepted to a certain extent it must undoubtedly be—throws a new difficulty in the interpretation of the affinities of animals, since before saying that an identity in some structural feature between the members of any two groups indicates their relationship, we have first of all to determine whether such similarity of structure is due to parallelism, or is inherited from a common ancestral type. As is so generally the case when any new theory is started, a host of enthusiastic writers have welcomed parallelism with acclamation, and have attempted to apply it in a number of instances where there is not at present sufficient evidence of its existence. We have been told, for instance, that the American monkeys have no relationship with their reputed cousins of the Old World; that whalebone whales are not allied to the sperm-whale and dolphins; that cats have no kinship with civets or other modern carnivores; and that the egg-laying mammals have been evolved from a reptilian or amphibian stock, quite independently of all other members of the mammalian class—their resemblances being solely due to parallelism. At present, we confess, we are totally unable to accept any of these conclusions, and we think it somewhat improbable that if the members of either of the above-mentioned pairs of groups had an independent origin, they would have presented such a similarity, both externally and internally, as we find to be the case. Still, however, it must be borne in mind that there is a considerable amount of evidence that the modern horses of the Old World and their extinct cousins of the New have been independently derived from earlier horse-like animals; and if this be confirmed, it will be one of the most remarkable known instances of parallelism, and will tend to show that many others may exist. Turning from these more or less problematical cases, we proceed to notice *seriatim* certain well-marked instances of parallel development as to the existence of which there can be no doubt; commencing with those displayed in external features, and then referring to such as are more deeply seated.

As regards external resemblances, it is, of course—and more especially so far as the lower animals are concerned—not always easy to distinguish between parallelism and mimicry. The above-mentioned instances of mole-like and hedgehog-like animals belong, however, clearly to the former category. The external resemblance existing between swifts and swallows, which have no sort of relationship to one another, likewise comes under the same head. Another striking instance is to be found in the assumption of a snake-like form, accompanied by

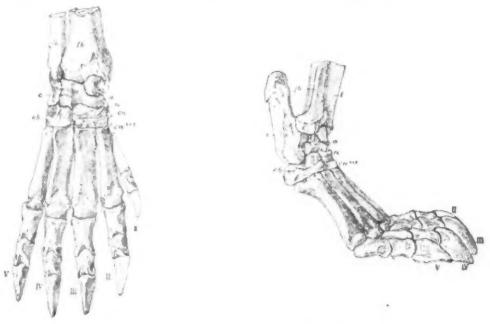
abortion of the limbs, by certain lizards, such as the familiar English "blind-worm," it being very doubtful, to our thinking, if this can be explained by mimicry. Still more remarkable is the external resemblance presented by whales and dolphins to fishes, and by the extinct fish-lizards (Ichthyosaurs) to both; such resemblances being clearly due to parallel development induced by the needs of adaptation to a purely aquatic life. If, moreover, it should prove that the whalebone whales have no connection with the dolphins, we should then have a far more remarkable instance of this feature, and one extending to internal structures as well as to external form. A similar remark will also apply to the case of the true seals and the eared seals, which it has been suggested may be of independent origin. The independent acquisition of wings by birds, bats, and the extinct flying dragons, or pterodactyles, may likewise certainly come under the heading of parallel development, so far as external form and the adaptation to a particular mode of life are concerned; although here, as in the instance of whales and fishes, the structural features, by the aid of which the adaptation has been brought about, are very different in the respective groups.

In respect to the teeth and dentition, many very well-marked instances of parallelism may be adduced. We have already referred to the similarity of the tusks in different groups of mammals; while in our last article we noticed the loss of upper front teeth, not only in the modern ruminants, but likewise in the peculiar even-toed ungulate known as *Protoceras*, of which the skull is figured on page 143. The rhinoceroses likewise show a gradual tendency to the loss of front teeth, resulting in the African forms in the disappearance of the whole series from both jaws. More remarkable, however, is the tendency to a complete loss of the whole of the teeth in certain groups of all the higher classes of vertebrates, as exemplified by all modern birds, by turtles and tortoises, as well as certain of the extinct flying dragons and fish lizards among reptiles, and by the great ant-eater, the scaly ant-eater, and the echidnas, or spiny ant-eaters, among mammals; in all of which teeth are completely lacking. Moreover, in many of these animals, such as birds, tortoises, and probably the toothless flying dragons, the beak-like jaws thus produced were sheathed in horn, thus showing a kind of double parallelism, viz., the loss of teeth coupled with the acquisition of a horny sheath to the jaws.

Another instance of parallel development afforded by teeth relates to the gradual heightening of the crown and the production of a flat plane of wear not only among several distinct groups of hoofed mammals, such as the elephants, horses, and ruminants, but likewise among the rodents. In a previous article, on "Teeth and their Variations," it was pointed out how these high-crowned teeth had been independently evolved in the three great groups of hoofed mammals, of which the above-named creatures are typical representatives; and it may be added here that the molars of horses and ruminants present a further evidence of parallelism in their assumption of a more or less decidedly crescent-like (selenodont) structure, the essential peculiarities of which are described in the article mentioned. This resemblance between the molars of horses and ruminants is, however, comparatively remote, but in the latter group and the camels these teeth are so alike as to require an expert to distinguish between them. Nevertheless, there is good evidence to show that the camels and the ruminants, if not also the chevrotains, have acquired their crescent-like molar teeth quite independently of one another, and it therefore yet remains for those writers who explain evolution by some mode of what they are pleased to call natural selection to account adequately for the similarity thus existing between structures of such totally different origin, when they could have been made equally efficient if unlike.

Passing from the consideration of teeth to that of limbs, we may mention that in previous articles we have already called attention to the remarkable similarity displayed in the mode by which the lower segments of the limbs of the even-toed and odd-toed hoofed mammals have been gradually elongated by the formation of a cannon-bone and the disappearance of either three or four of the lateral digits; the cannon-bone in the horse consisting of but a single element carrying one digit, while in the ruminants it comprises two united elements supporting a pair of toes —this being clearly a case of parallelism in development attained by a slightly different modification of principle. The parallelism does not, however, stop here, since (as shown in our article entitled "Giant Birds") an essentially similar type of cannon-bone has been produced in birds; only that in that group (with the exception of the ostrich) three long bones enter into its composition, which is further complicated by the addition of a bone from the ankle above. Seeing that in all these groups the parallelism has been arrived at by a different structural modification, the explanation of its mode of evolution is much less difficult than in the case of the molars of the camels and ruminants, where, as we have seen, the structure is practically identical.

Recent discoveries in North America have brought to light the existence of a kind of secondary parallelism among certain peculiar mammals which may be included among the hoofed or ungulate division of that class. In



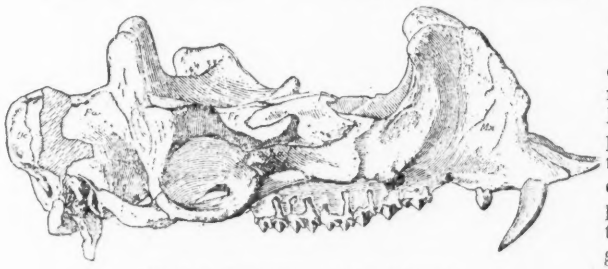
Front and side views of the hind foot of *Artionyx*. (After Osborn.)

the even-toed group of that division, as exemplified by the pigs and ruminants, it is the third and fourth digits which are symmetrical to one another, and tend to develop at the expense of the others; while in the odd-toed group it is the third or middle digit which is symmetrical in itself and tends to an ultra-development. Now there are certain extinct creatures which, while having claws instead of hoofs at the ends of their toes, yet are so closely related to the hoofed mammals that their separation therefrom is almost impossible. In one of these, which has long been known in Europe as the chalicothere, the third digit, as in the odd-toed hoofed mammals, is symmetrical in itself, and larger than either of the others; whereas in the newly-described animal known as *Artionyx* the third and fourth digits resemble those of the even-toed division of the hoofed mammals in being larger than the others and symmetrical to a line drawn between them. While, therefore, we have clearly a parallel development on different lines between the odd and even-toed hoofed animals in the development of a cannon-bone, these extinct clawed, hoof-

like mammals (for want of a better expression) show a parallel parallelism (to coin another expression) which, if it had continued, might have resulted in the development of a two-clawed ruminant and a two-clawed horse. To our thinking, this is indeed one of the most curious phases of development yet discovered.

In addition to the parallelism in the evolution of their molar teeth and limbs, the hoofed mammals likewise exhibit the same feature in regard to the second vertebra of the neck. As most of our readers are probably aware, the first or atlas vertebra of the neck turns with the head when the latter is moved sideways, the axis of rotation being formed by a process—the odontoid process—arising from the second or axis vertebra, and projecting into the central hollow of the atlas. Now in pigs and likewise all the primitive hoofed mammals, the so-called odontoid process is (as in ourselves) in the form of a flattened peg. On the other hand, in the ruminants, the modern horses, and the camels, which, as we have seen, represent three distinct *phyla* of the order, this peg has, however, become modified into a spout-like half-cylinder, which must clearly have been separately evolved in each of these three groups. It is true that such a half-cylinder affords a far better basis of support for a heavy skull than does a mere peg; but the curious part of the matter is why these half-cylinders should be so exactly alike in the different groups, seeing that as it would not be difficult to design some other structural modification by which the same end might have been attained, there is no necessity for their similarity.

Our last example of parallelism will be drawn from two groups of extinct North American hoofed mammals, to which brief allusion has already been made in our article on "Tusks and their Uses"; the one group being known as



Skull of *Protoceras*. (After Osborn.)

uintatheres, while the second is represented by a single species to which the name of *Protoceras* has been applied. Now, although the uintatheres have five-toed feet approximating in structure to those of elephants, while in *Protoceras* each foot approximated to the ruminant type, in both groups the skull, as shown in the accompanying figure, was armed with several pairs of large, irregular, bony processes, which during life may have been sheathed in horn; while in each case a pair of long tusks projected from the upper jaws, which were totally devoid of front or incisor teeth. Had such skulls been discovered without any indication as to the nature of the limbs with which they were associated, they would inevitably have been assigned to the same group of animals. The resemblance existing between them is, however, clearly due to parallel development, and we are thus shown another striking instance of caution necessary in endeavouring to determine the affinities of extinct animals from the evidence of incomplete remains.

Our last instance of parallelism has been already alluded to in an article entitled "The Oldest Fishes and their Fins," in the course of which it was shown that while both the most ancient birds and the oldest fishes had long tapering tails with the joints of the backbone gradually diminishing in size, and each carrying either a pair of feathers or a pair of fin-rays, in all the modern representatives of the former group, and in a large section of the latter, the end of the vertebral column has been aborted into a composite bone, from which either the feathers or the rays of the tail diverge in a fan-like manner.

In conclusion, we may say that although there is no very great difficulty in satisfactorily accounting for external parallelism obviously due to the necessity for adaptation to a particular mode of life, or in explaining those instances where a particular result has been brought about by different methods, yet when we find precisely similar structural modifications in different groups of animals which clearly cannot be traced to a common ancestry, and for which equally efficient substitutes could be readily suggested, we are fain to confess that the ordinarily accepted explanations of evolution appear to us altogether inadequate. Putting this aspect of the matter aside, our readers will, however, see from the imperfect sketch given above what an important factor in evolution parallel development really is, and how largely it is likely in the future to modify our present views as to the mutual relationships of animated nature.

GALLS AND THEIR OCCUPANTS.—II.

By E. A. BUTLER.

(*Continued from page* 128.)

NO other English tree is such a favourite with gall-flies as the oak; nor is this to be wondered at, since the same tree is by far the most popular in the insect world at large. About fifty species of gall-flies are known to attack the two kinds of oak tree which are found wild in this country, and many others are dependent upon the oaks of other lands. No part of the tree is exempt from attack. The unexpanded leaf-buds, the full-grown leaves, the male catkins, the acorns, the young twigs, the leaf-stalks, the bark, and even the roots, all have their special inhabitants, each producing its own characteristic excrescence. Many of these galls are small and insignificant, and hence not generally known; but in addition to these inconspicuous things, the oak supplies both the conspicuous marble galls already described, and also several other very well-known kinds. One of the most familiar is the "oak-apple," a soft, exceedingly succulent, somewhat irregularly spheroidal mass of tissue found on the young twigs in the position of a terminal leaf-bud. Its colour is pale yellowish or brownish white, dashed with rosy pink where the sun has shone most directly upon it. It does not turn hard as the marble gall does, but, though like a juicy apple at first, becomes ultimately a light brown, dry and spongy mass, resembling a small shrivelled apple. When picked in this dried condition it is found to be astonishingly light.

FIG. 4.—Section of Oak-apple, showing cells of Gall-flies.

The oak-apple results from the punctures of the gall-fly named *Andricus terminalis*. In the centre of its mass it contains a number of hard, oval, woody cells (Fig. 4), each the home of a gall-fly larva. It is not therefore like the marble gall, which in its normal condition

is a one-celled structure; but it is naturally many-chambered, and normally provides accommodation for a family of gall-flies, irrespective of parasites, which of course also occur in it. This is a springtide gall, the fly issuing at the end of May or the beginning of June. The galls, being fully formed by the end of May, and being just at that time by no means inelegant objects, were formerly in considerable request as buttonhole decorations on the 29th of May, "Restoration Day," whence their popular name of "King Charles's apples." When the galls have yielded up their rightful occupants they still do not cease to be useful. Those resplendent green beetles, the rose-chafers, find them out in June, and devour the softer parts, leaving the hard oval cells exposed to view in a cluster at the end of the twig. But even if the softer part be not eaten off by insects, it still disappears, flaking away piece by piece, so that if the galls are looked for in the winter, only the clusters of hard cells will be found. From these, however, parasites often issue, sometimes even as late as the second year.

Another gall-fly, called *Dryophanta scutellaris*, makes spherical galls on the under side of the oak leaves; these, when young, bear considerable resemblance to the marble galls, which, as we have seen, are the produce of quite a different insect. When mature, however, they could not be confounded; the present galls become yellowish, and finally red like a ripe fruit on the side turned towards the sun, whereas those of *Cynips Kollari* become dark brown and woody. Their situation, too, on the *leaves* instead of the twigs, helps to distinguish them; the flies, moreover, mature later than the generality of those from the marble galls. When these galls are fully grown they are about the size and shape of cherries, and this, coupled with their rosy sides, has acquired for them the name of "cherry galls."

Yet another globular gall may be found pretty frequently on oak leaves, the so-called "berry gall," smaller than the last, semi-transparent, and very juicy. Save for its colour, it reminds one of a currant, and hence, when found growing on the pendant catkins of the male flowers of the oak (for it occurs in this way as well as on leaves) it gets the name of "currant gall." The name of its founder is *Spathegaster baccarum*. The growth of this gall is remarkably rapid; only about three weeks elapse between the first appearance of the gall and the emergence of the fly. It is only in May, therefore, that we should expect to meet with the galls, for after the escape of the insect they shrivel up and soon disappear. If any are to be seen later they will be found to be inhabited by parasites.

Fig. 5.—Artichoke Gall of Oak Tree.

One of the most remarkable of oak galls is that known as the "artichoke gall" (Fig. 5), inhabited by *Aphilothrix gemmæ*. Until a section of the gall is made, and the insect found therein, it seems impossible to believe that we have before us anything more than a normal vegetable production. Galls can hardly be regarded in any other light than as pathological features in the life-history of the plant, tumours, as it were, resulting from a deranged condition of the tissues; and yet in such instances as the present there is no trace of any irregularity of form, or shapelessness and malformation, such as we should expect in a diseased product, but all is beautifully symmetrical and regular, the parts being arranged according to the ordinary laws of plant structure. Of course there are galls which are obviously abortions and monstrosities, and which greatly disfigure the plant on which they occur, but that is not the case with any of those we have yet considered, and least of all with the artichoke gall. It is of a pale whitish or yellowish-green colour, reminding one very forcibly of an artichoke flower in miniature. It is in reality a modified bud, but still one modified without distortion; and therefore there are a number of leafy scales overlapping one another perfectly regularly, and making a rather stumpy conical body, like the head of a thistle. On turning back these leaves, an oval object is seen standing erect in the centre; this is hollow, and is the abode of the grub of the gall-fly, hence it is the really important part of the gall, the rest being merely accessories. About the end of August this hollow chamber falls to the ground, and remains there throughout the winter, the insect issuing from the gall during the next spring.

A large woody gall is sometimes found on the roots of the oak tree, just where they leave the trunk, and therefore only slightly below the surface of the ground. This is occupied by a gall-fly called after the locality in which the gall occurs, *Aphilothrix radicis*. In form it is something like an oak-apple, but it is much harder and more fibrous; moreover, it is usually much larger, being indeed the largest of our British galls, and attaining sometimes the size of a man's fist. Like the oak-apple, it is tenanted by large numbers of the flies, each inhabiting a hard-walled oval chamber. The colonies are sometimes extremely extensive, giving one some idea of the high degree of fecundity often attained by gall-flies. Westwood records having obtained as many as eleven hundred specimens of the rightful occupant from one gall of this kind, which was about five inches long by one-and-a-half broad. The flies appear in April.

Galls have been for a long time known to science, as indeed, from the conspicuousness and attractive appearance of some of them, might have been expected. Theophrastus, a Greek philosopher who lived about three hundred years before Christ, the author of one of the earliest botanical treatises extant, mentions them; and Dioscorides, a writer on medicine, who lived some four hundred years later, discusses their medicinal virtues in a work which became one of the standard medical treatises of the Middle Ages. In consequence of the tannin they contain, they were valued for their astringent properties, and were largely used as a remedy for affections of the mucous membrane of the mouth, and other similar complaints. By the ancients they were always treated as purely vegetable products, and the true nature of the connection between the plant, the gall, and the insect does not seem to have been suspected till comparatively recent times. Even Bacon speaks of oak-apples as an "exudation of plants joined with putrefaction." The great difficulty about them was always felt to be the presence of an insect inside a portion of a plant, without any apparent means of entrance; this greatly puzzled the learned, and led to many curious conjectures as to the possible method of its introduction. It was evident that the insects were hatched in the place in which they were

found, but the question was, how could the eggs, supposing the insects to have originated from eggs, get into the centre of a mass of vegetable tissue, when no pathway thither could be discovered? The difficulty was of course enhanced by the prevalent belief that the galls were a natural product of the plant, not diseased excrescences, and were, in fact, a sort of fruit of the plant, as much so as its ordinary and legitimate fruit.

Many explanations of the mystery were proposed. One theory was that the eggs were derived from the ground, and that they passed into the roots of the tree, and along its vessels with the ascending sap, till ultimately they reached their proper receptacle, which had been already provided for them, and in which they were destined to mature. Or, again, it was thought that there might be germs floating in the air, which at last came in contact with some part of the plant, to which they adhered, and then the plant, pursuing its natural growth, grew round the germs, and finally enclosed them. Another explanation was offered to the effect that, though the insects themselves belonged to the animal kingdom, yet they were a direct product of the *vegetable* life which animated the plant in which they were found; that in fact they did not arise from eggs or germs, but that whatever forces those were that produced the growth of the ordinary parts of the plant, leaves, flowers, fruit, or what not, the same forces also led to the formation of the insects found in the supposed secondary fruits. Indeed, it was gravely asserted that one of the express purposes of the formation of fruits at all was that they might beget and nourish these insects found inside them. And yet, in this last supposition, absurd as it may seem, we have—since the sole purpose of the gall, so far as we can see at present, is to nourish and shelter the contained insect—a sort of half-truth obscured by the idea that the galls are a natural product of the tree, of the nature of a fruit, but destined to produce and perpetuate an animal offspring instead of a vegetable one. In the fact that the oak tree produced not only its ordinary vegetable fruit, the acorn, but also an animal fruit as well, the gall-insect, some saw a reason why the ancients dedicated the tree to Jupiter. And then, lastly, there was of course also the doctrine of spontaneous generation, which could be fallen back upon when every other explanation failed.

The mystery thus attaching to the gall and its imprisoned inhabitant served not unnaturally to connect them with occult science, and they were used for purposes of prognostication. They were the repositories of Nature's secrets, and it was according to the form their occupants assumed that the character of the forthcoming season would be determined! The unpierced galls were believed to contain either a fly, a spider, or a worm. The fly was of course the fully-matured gall-insect, and the worm was its larva; what the spider was it is not so easy to see, unless galls containing mites or beetles were referred to. However, if on opening the gall a fly was found within, it was a presage of war; if a worm, the price of the commodities of life might be expected to rise; but if the prisoner turned out to be a spider, this was an ill omen indeed, and the season would bring a dismal record of pestilence and death.

Of quite a different character from any of the galls we have already considered are those pretty adornments of oak leaves called "spangles." They are flattish, circular bodies, of a reddish or brownish colour, and attached in considerable numbers to the underside of the leaves (Fig. 6). They are fastened to the leaf at one point only, by means of a minute stalk, so that they look something like a tiny fungus, for which in fact they were at one time mistaken. They are formed by various species of a genus called *Neuroterus*, and the economy of the insects is very different from that of the species we have already considered. They are long in coming to maturity, and indeed scarcely begin their development as long as the galls remain attached to the tree. The spangles appear in the autumn, and after a certain period of growth become detached from the leaves through the severance of their short little stalk, and fall to the ground, leaving only a small spot on the leaf to show whence they have come. They lie about at the foot of the tree through the winter, absorbing moisture from the damp ground, and swelling considerably. Thus, although severed from the tree that produced them, and cut off from its supplies of sap, they are yet able to remain succulent, and indeed to become still more so than when on the leaves, and so can contribute to the growth of their occupants. The grubs, therefore, which are exceedingly minute till after the spangles fall, feed chiefly during the winter, and the flies become mature by about February or March in the following year. Two of the best known of these spangle-galls are those of *Neuroterus lenticularis* and *numismatis*. The former are lens-shaped, hairy galls of a reddish colour, and the latter are of a golden brown, like little silk buttons. Both are gregarious, that is to say, large numbers of the galls are found on the same leaf, sometimes so close together as to interfere somewhat with one another's growth.

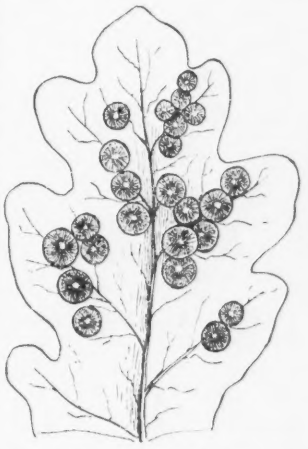


Fig. 6.—Portion of Oak Leaf with Spangles *(Neuroterus lenticularis)*.

An exceptional interest attaches to these oak spangles and their occupants, inasmuch as they constitute one of the best instances of the remarkable phenomenon of heterogamy, or that particular variety of the alternation of generations which consists of the regular succession of two types of creatures, the one consisting of both males and females and producing eggs in the ordinary manner, the other consisting of females only and producing eggs without impregnation, the whole series constituting in reality but one species. We have spoken above of the flies that issue from the currant galls and spangles by the names by which they are generally known, and therefore it will be seen that they are referred not merely to different species, but even to different genera; it remains now to point out that they are in reality not two distinct insects, but only two stages in the history of one. It will be remembered that the currant galls and spangles do not appear at the same season, but that the former are very short-lived, lasting for less than a month, while the spangles appear later in the year, after the currant galls have passed away, and last much longer. Thus one might naturally wonder what becomes of the inhabitants of the currant galls during the other eleven months of the year, and what condition they are in, since they only take one month to pass through their entire metamorphosis. It now appears that during the rest of the year they exist in another form, as inhabitants of the spangles. The discovery of these remarkable facts was first announced by

Dr. Adler about fifteen years ago, and they have since been confirmed by Lichtenstein and others.

In brief, therefore, the entire life-history of this insect is as follows. If we start with the currant galls, either on the leaves or on the catkins, these produce at the end of May the fly which is called *Spathegaster baccarum*. Both males and females are produced, and the females, after mating, pierce the oak leaves and deposit their eggs in the punctures; but from these punctures there do not proceed, as might be expected, a further crop of currant galls, but, on the contrary, a collection of spangles. These, after maturing during the latter part of the summer, drop to the ground in October, there swell and develop the contained insect, which is now known as *Neuroterus lenticularis*, and emerges in the following spring. This spring crop of flies, however, are all of the same sort; no distinction of sex can be observed and no males are known. They all produce eggs which they deposit in punctures on the leaves or on the catkin stems, and from these punctures are produced again, not spangles like those the flies came from, but the currant galls of *Spathegaster* with which we commenced the series. From these issue, as before, male and female insects belonging to *Spathegaster baccarum*, and the whole cycle is gone through again the next year. Thus in the course of one year the same insect appears in two successive phases, one of which has both males and females and the other females alone, and which make galls so utterly unlike in character as to have been referred to two distinct genera.

The phenomenon of parthenogenesis occurs in many other species of gall-flies, but is not necessarily associated with that of dimorphism, *i.e.*, the occurrence of the same insect in two distinct forms, and even in some cases, where the males are known, their number bears an exceedingly small proportion to that of the females. The marble gall is an excellent illustration of the former peculiarity. Mr. F. Smith, the zealous hymenopterist, obtained in the year 1857 about a bushel and a half of Kollari galls from Devonshire, as they were not then to be found in the neighbourhood of London. At the beginning of April in the following year flies began to emerge, and continued to do so for two months. Altogether about twelve thousand specimens were obtained, and all as they came out were examined; every one proved to be a female. About sixty of the galls had been placed in separate boxes, and when the flies issued from these they were conveyed to different localities in the neighbourhood of London and placed on low oaks and hedges. On revisiting these spots in August, Mr. Smith found, in about two-thirds of the cases, galls on the trees on which he had placed the flies, but on none in the vicinity. These galls were again collected, and the next year the flies bred from them; these, again, proved to be females. They were at once taken to other and isolated spots and left on trees as before. Again, in the summer galls were found in the same proportion as before. Thus it was shown that the insect could breed from year to year without males at all.

The same observer obtained from one example of a root-gall one thousand two hundred flies of *Aphilothrix radicis*, all of which proved to be females. Hartig, again, speaks of having examined fifteen thousand specimens of different kinds of gall-flies without ever discovering a male. On another occasion, from an enormous collection of twenty-eight thousand galls of a species resembling the cherry gall, he obtained between nine and ten thousand flies, all of which were females. In fact, one may state generally, that none of those species of oak gall-flies that are single brooded are known to possess a male at all.

(*To be continued.*)

THE SUN AS A BRIGHT-LINE STAR.

By Miss A. M. Clerke, *Authoress of "The System of the Stars" and "A Popular History of Astronomy during the Nineteenth Century," &c., &c.*

By far the most remarkable pair of lines in the solar spectrum are those known, since Fraunhofer observed and named them, as "H" and "K." They lie high up in the violet—so high up, that they can be much more easily photographed than seen. Intense absorptive action is indicated by the depth and extent of their central obscurity, which seems besides as if veiled in diffusive shadow. They are distinctive of the metal calcium, which, however, emits them conspicuously only under the stress of powerful electrical excitement; thus they may be provisionally termed "high temperature lines." In the near neighbourhood of the sun they are ubiquitous. The substance from which they emanate rises to the summits of the loftiest prominences; it enters largely into the composition of the chromosphere; it shines even in "white prominences" and faculæ. These facts, although they have been securely established by the recent investigations of Prof. Hale and M. Deslandres, do not easily fit into the narrow framework of existing theories. Yet of their profound importance to solar and stellar physics there can be no doubt; nor can they receive too close or too careful consideration.

There is evidence, to begin with, that the lines in question (of wave-lengths 3968 and 3934 tenth-metres) claim, in the sun, a three-fold origin. The wide dark bands in the ordinary solar spectrum are produced by an absorbing layer necessarily cooler than the photosphere, and probably in its immediate neighbourhood. Calcium-vapour seems here to be affected very much as it is in the core of the electric arc, where the H and K lines, just traceable in a magnesium-flame, come out strong and diffuse. Their centres are marked (as Prof. Hale has noted) by thin, extra-black lines, possibly due to the arresting action of the chromosphere. The chromosphere, as the readers of Knowledge are well aware, completely envelopes the solar globe with a tossing incandescent ocean. The bright calcium-lines forming part of its spectrum reach us clear, sharp, and undimmed from the edge of the sun. Hence, evidently, the Fraunhofer absorption takes place at a lower level than that at which they are generated. Otherwise they would be hopelessly cut off from transmission to outer space: they would be swallowed up by the superposed, powerfully absorbent vapours. And the same reasoning applies, of course, to the chromospheric rays of hydrogen. The H and K of the chromosphere are of the same character with the lines radiated by calcium during the passage of the electric spark. They might, then, be supposed to indicate a higher temperature than that corresponding to the dark bands in the solar spectrum. But in our present inability to discriminate between effects of simple heat and effects of electrical agitation, no definitive judgment can be formed upon this point. It appears certain, on the other hand, that the chromosphere is not hotter than the photosphere; for if it were, the calcium-lines of the ordinary solar spectrum should be reversed all over the sun—that is to say, a fine, brilliant ray would shine at the middle of each dusky band. In fact, however, there are (as we have said) fine, *dark* rays in these positions, most likely as the result of a two-fold light-stoppage. But with a chromosphere of overweening radiative intensity we should get, instead of double absorption, absorption plus emission.

In faculæ, on the contrary, reversals, which the chromosphere is apparently incompetent to produce, do

actually take place. These extraordinary objects display a continuous, in addition to a bright-line spectrum. Their constitution would thus seem to be almost identical with that of "white prominences." They must be enormously hot—hotter, certainly, than the photosphere, since certain ingredients of their light stand out vividly against the background furnished by it. Prof. Hale has, in fact, succeeded in photographing them, through their H and K emanations, on all parts of the sun's disc. Their unexpected importance has thus been directly brought into view. Far from being confined to the vicinity of spots, faculæ are now perceived to cover the spot-zones with an almost unbroken network, and to occur sporadically even to the very poles. They show no marks of absorption by the cooler chromospheric gases, and may hence be inferred either to attain to a higher level, or to be in some other unknown way protected against their inroads. For it must be admitted that Kirchhoff's law, although true so far as it goes, does not at all completely express the relations of emission to absorption in celestial bodies.

The effectiveness of facular light in modifying the spectrum of the sun, taken as a whole, obviously depends upon the proportionate amount of it present. Increase it sufficiently, and its bright-line ingredient will eventually become first distinctly perceptible, and at last conspicuous. Under these circumstances, then, of augmented facular development, the sun observed, for example, from Sirius would appear as a bright-line star. And it might very well be that his appearances under that aspect would be periodical; for the faculæ emblazoning his surface become much more numerous and intense with the growth of sun-spots, and attain, concurrently with them, a maximum once in about eleven years. Hence the periodicity of the sun might, with a slight intensification of existing conditions, be ascertained from distances at which he would be reduced to the status of an insignificant point of light.

This is no mere theoretical inference. On occasions when faculæ were unusually numerous and lustrous, M. Deslandres has actually succeeded, by treating the sun as a star—that is, by admitting into his apparatus the whole of its rays simultaneously—in photographing vivid reversals of the calcium-lines. The sun was then, *pro tanto*, a bright-line star. The aggregate of his light, in other words, contained evidence of gaseous emissions. But other stars of the solar type are presumably subject to similar vicissitudes. Capella, Arcturus, Aldebaran, undergo, we are entitled to suppose, cyclical agitations marked by the fluctuating encroachments upon their photospheres of spotted and faculous areas. These ought, at least in some cases, to betray themselves spectrographically by the creeping-in of fine lines of light into the dense bands of calcium-absorption characterizing all solar stars. But the anticipated appearance might have to be long waited for. Numberless negative results would probably put a severe strain upon the patience of the investigator. A single positive one would, however, be no trivial success. It would imply the opening of a hitherto closed door. It would give the means of determining the flow of change in suns inconceivably distant and immeasurably vast. Through the prosperous issue of such a research the spot-maxima, say, of Arcturus might come to be known more accurately than those of our own "bright particular star." The study of solar periodicity, moreover, could hardly fail to reap advantage from comparisons of corresponding phenomena in sun and stars; and the problem of stellar variability might even be brought a step nearer to a solution.

In Mira Ceti, and probably in other periodical stars of the same class, H and K are no less dark and broad than they are in the sun; but they appear to originate under very different conditions. The evidence regarding these depends upon the circumstance that H is double. Both hydrogen and calcium have commonly a share in its production. A member of the hydrogen series, in short, falls so near the calcium ray that when either is much widened it conceals the other. Thus the diffuse H in the spectrum of white stars like Vega appertains to hydrogen. The presence and state of the nearly coincident calcium-line can only be inferred from the rule of its approximate correspondence with its twin K, which, in Vega, has shrunk into insignificance. Conversely, the hydrogen H, if present at all in the sun's absorption-spectrum, is plunged so deep in the shadow of the calcium-band as to be undistinguishable. In order, then, to avoid these ambiguities, K is taken as the representative of the calcium pair; and its value as an index to stellar constitution, pointed out thirteen years ago by Dr. Huggins, has been in no small degree heightened as the result of recent discoveries.

Now the Harvard College "spectrograms" of Mira show six brilliant hydrogen-rays, namely, G, *h*, with four of the ultra-violet sequence. But the intermediate H is invisible. Few, however, will venture to assert that the continuity of the harmonical progression is really broken. The gaseous molecules cannot conceivably be agitated in such a manner as to cause them to drop out this particular quality of vibration. There is, moreover, an obvious reason for its apparent absence.* It is obliterated by the strong calcium-band falling near its place in the spectrum. This implies that the absorbing calcium-vapour is situated between our eyes and the glowing hydrogen, the radiations of which, so far as they come within its grasp, are cut off by it. In other words, the region of this particular kind of absorption, which, in the sun, is sunk below the region of bright-line emission, overlies it in Mira. There is, apparently, no escape from this conclusion. We are then forced to admit that what might have been taken for the natural arrangement in order of vapour-density does not invariably prevail in stellar atmospheres. What the counteracting influences may be, we know not; we can only note the effects of their exertion. These certainly deserve very close attention; for there are few facts in astro-physics more curious than the seemingly inverted positions of the absorbing and emitting strata in the sun and Mira. Yet the subject has attracted up to the present singularly little attention. Data for its profitable discussion are much needed, but are not forthcoming. They could, nevertheless, be easily procured with the aid of very moderate spectrographic appliances. A few photographs of the spectra of long-period variables showing bright lines, such as χ Cygni, R Andromedæ, R Leonis, and many more, would at least tell whether the eclipse of the hydrogen H by the calcium H is a peculiarity common to the class. A few additional photographs would answer the further question as to whether the eclipse is a permanent condition, or merely a transient phase in each individual star.

Nova Aurigæ was unique among sidereal objects in its display of bright calcium-lines. Nor did they persist even here. They became extinct during the temporary invisibility of the star during the summer of 1892. Yet, considering the predominance of H and K in the spectra of solar appendages, and their easy reversibility, their inclusion as vivid rays might have been anticipated in a great number of emission-spectra. They remain, nevertheless, hitherto unrecorded in nebulæ, bright-line stars,

* See an article by the present writer in *The Observatory*, Vol. xi., p. 84.

and variables of short or long periods. This general statement of their absence, however, since it rests upon purely negative evidence, may need more and more qualification with the advance of photographic research.

Note by the Editor.

[Everyone will admit the extremely interesting nature of the questions discussed in Miss Clerke's paper, and though we may not all agree with her in the very definite conclusions she draws as to the existence of stratified layers of absorbing vapour about the sun and stars, and as to their relative positions and temperatures, the mere discussion of such questions is likely to be instructive. I wish, however, to guard myself, as Editor, from being supposed to put forward her views as generally received.

Personally I am disposed to doubt the existence of the shallow layer of cool vapour above the photosphere which Miss Clerke, as well as other writers, have assumed to exist in order to account for the dark lines of the Fraunhofer spectrum. Nearly all the lines of the Fraunhofer spectrum, whether they are broad or thin, hazy or dark, do not change in breadth and intensity in passing from the centre of the sun's disc to the solar limb; whereas with a shallow absorbing layer, we should expect to find much greater absorption near the limb, where the distance of transit obliquely through a shallow stratum and the consequent absorption would produce a greater blackness of the lines than at the centre of the solar disc, where the light has only to pass through the vertical thickness of the stratum.

Prof. Hale's photographs, as well as direct observations, prove that in the region of the faculæ the H and K lines are very bright compared with the light of other wave-lengths emanating from the sun's disc. If the vapour which absorbs the Fraunhofer lines extends to a great height above the faculæ, we should expect it to reduce the light of the faculæ as well as the light of the photosphere. To take a simple example, let us suppose that it halves the brightness of light of K wave-length and reduces the light of the continuous spectrum of the photosphere so that a line is produced in the K region which appears relatively dark compared with the continuous spectrum on either side of it. But the K light from the faculæ may be so bright that after half its brightness is absorbed it still appears bright compared with the continuous spectrum on either side of it. It will be noticed that it is only the brighter central part of the K line that gets through and appears bright as compared with the spectrum on either side of it; the fainter wings of the bright line are so reduced as to be lost, and at a very little distance they are reversed so that the central part appears bright in the midst of a nebulous dark band—the intensity of absorption evidently to some extent depends on the difference of temperature of the emitting and absorbing medium—and it seems probable that the proportion of light absorbed from the hot photosphere would be greater than the proportion absorbed from the cooler incandescent gas above the photosphere. The absence of the H line in the Mira spectrum cannot be taken as proving that the cool calcium-vapour is above the glowing hydrogen. The absorbing calcium-vapour may be in the same region, and intermixed with the glowing hydrogen. It is only necessary to suppose that hydrogen glows with H light at a lower temperature than calcium-vapour, and that each foot in depth of calcium-vapour absorbs the H radiations emitted by the preceding foot of hot hydrogen.

The many vertical eruptions in the chromosphere and corona, as well as what we know of the law of gaseous diffusion, seem to render it improbable that there is any horizontal stratification of gases into thin layers upon the sun, and to lead to the conclusion that the appearance of stratification presented by the lower regions of the chromosphere and by the even surface of the photosphere is due to the difference of temperature at different levels, the isothermal surfaces about the heated nucleus of the sun being a series of concentric spheres.—A. C. Ranyard.]

Mr. M. Glover, of 124, Stephen's Green, Dublin, writes, enclosing two photographs of a mock sun which he observed at Dublin on the evening of the 30th of June last. The phenomenon was first noticed about 7.30 p.m., and remained visible for about twenty minutes. The photographs were made with very short exposures, and show the sun's disc seen through cirro-stratus cloud, with a spurious image of the sun also seen through cloud at a distance of about 25° from the sun, and at about the same altitude above the horizon. A similar image was seen by Mr. Glover on the left hand of the sun, but it had faded away before the photographs were taken.

Prof. W. W. Payne, of Carleton College, Northfield, Minnesota, is about to issue a new astronomical magazine, to be entitled *Popular Astronomy*. It will be issued monthly, except in July and August, and is intended to meet the wants of "amateurs, teachers, students of astronomy and popular readers."

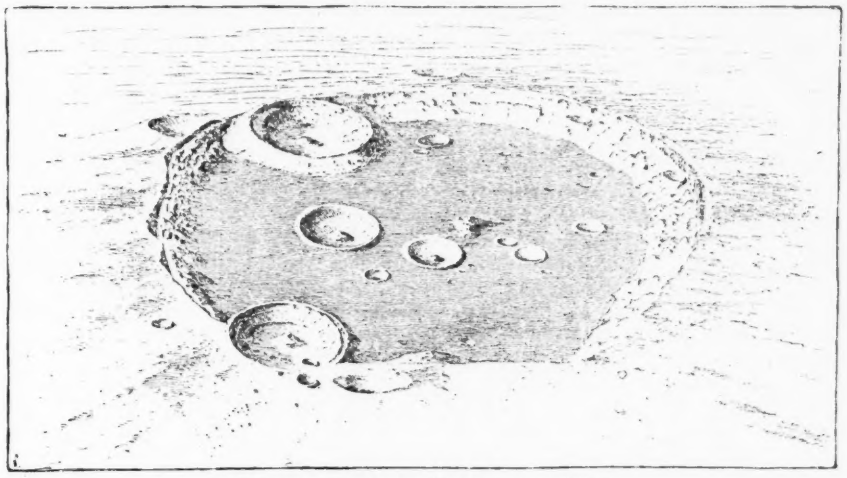
Scientific investigators are beginning to consider very seriously the problem of a too rapidly accumulating literature. The essence of scientific research is the marshalling and comparison of facts, but as each of the multiplying class of investigators brings in his contribution the need is felt of some authoritative organization of these units into a larger body of knowledge. Scientific publications overlap excessively and there is no reliable index, so that unless a specialist is so happily situated as to see all of the great number of periodicals which may contain work affecting him, he goes in constant danger and dread of missing what may be of vital importance to his own enquiries. Even if he has access to all the literature, the time he will have free for original work is considerably diminished by his study of the work of other men, and by his searches through the chaos of published papers. So much is this the case that it would not be hard to point to men who are not so much scientific enquirers as scientific scholars, devoted almost entirely to keeping pace with the infinite ramification of current work. A clearer definition of the province of certain scientific magazines may possibly do a little to diminish the difficulty, but the trouble will only be fully met by the establishment of subject indices. One of our contemporaries, which is always ready to advocate any scheme for getting money from the Government, urges that a general subject Index of Science should be established on a generous scale. It seems to us that the red tape methods of officialism are not suitable for such an undertaking, and that the work would be more satisfactorily accomplished if each of the learned societies would publish annually or quarterly a full subject index in its own department; and that the best results would be obtained if each specialist could be induced to give with each book and paper issued full references to the authorities he has used.

Local telegrams are now transmitted through pneumatic tubes in most of the principal cities of Great Britain. At present about 50 miles of such tubes are in operation, requiring an aggregate of 400 horse-power and transmitting a daily average of 105,000 messages, or 30 millions annually. More than half of these are in London.

THE GREAT LUNAR CRATER TYCHO.

By A. C. Ranyard.

THE late Prebendary Webb used to speak of Tycho as the metropolitan crater of the moon. Though by no means the largest of the lunar craters, it is one of the most striking features of the lunar landscape, especially when the moon is near to the full, and the shadows of the mountains have all disappeared. The crater of Tycho is then seen as a conspicuously white spot, from which radiate in all directions a great number of whitish rays that extend over more than a third of the visible hemisphere of the moon, indicating that the crater has been the centre of a colossal disturbance which seems to have shattered the lunar crust in all directions. We have, as far as I am aware, no evidence in the terrestrial geologic record that a corresponding cataclasm has ever similarly shattered the earth's crust; but our terrestrial volcanoes are puny things compared with the giant craters of our smaller companion planet.



Clavius, from a drawing by Mr. G. K. Gilbert.

As might be expected, the strange phenomena presented in so unparalleled a degree by Tycho have been a fruitful stimulus to speculation as to the origin of the lunar craters and the radiating systems of rays with which many of the moon's craters are evidently intimately connected. Many able men have doubted whether there is any true analogy between terrestrial volcanoes and the gigantic lunar ring mountains and circular depressions which we ordinarily speak of as craters. The ring of Tycho is 54 miles in diameter, and the great crater Clavius, which lies to the south of it, is more than 140 miles in diameter; but Clavius is by no means the largest of the lunar craters. If the lunar Apennines and the other mountains forming a broken ring round the Mare Imbrium are the remnants of a crater, it must have had a diameter of over 600 miles, while the largest terrestrial craters are not more than 15 or 16 miles in diameter.* Vesuvius and the Monte Sumna would appear as insignificant little hills if they were dropped into the centre of the crater of Tycho, whose ring wall towers to a height of 17,000 feet above the plain it encloses.

Clavius, from a photograph by MM. Henry.

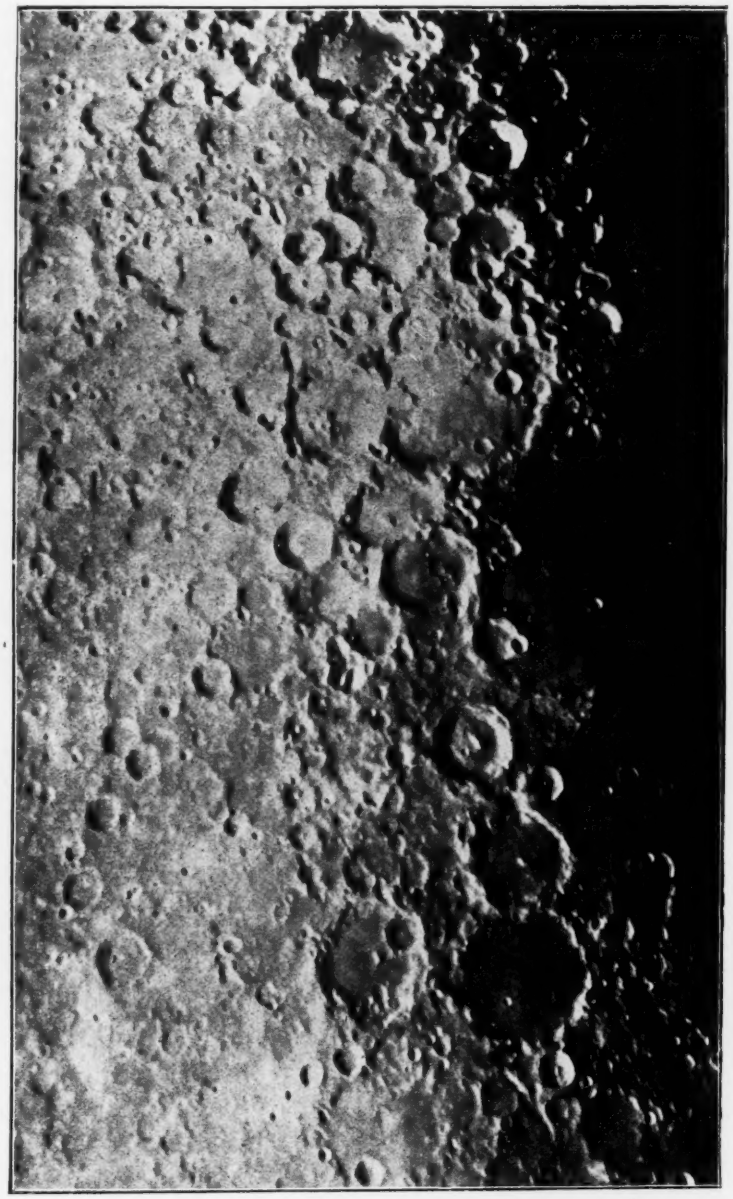
Robert Hooke compared the lunar craters to the cup-shaped pits formed on the surface of boiling mud by escaping vapour, and the idea has been a fascinating one to many minds since his day, though it needs but little consideration to recognize that bubbles or blisters formed in a plastic material on a scale corresponding with that of the lunar craters would rapidly sink down and be obliterated.

The Mare Nubium.

Mr. S. E. Peal has ingeniously advocated a theory which seems to me almost equally untenable. He assumes that the lunar surface consists entirely of ice, and that the

* According to Mr. G. K. Gilbert, in a paper published in the *Bulletin of the Philosophical Society of Washington*, vol. xii., p. 247, (1) the old crater containing Lake Bombon, Isle of Luzon, is

craters and pit-like depressions are due to the action of hot springs which have not flowed continuously, but that water has from time to time issued from vents in the soil, and has melted the ice above the vent; the water is then supposed to have flowed back to the warm interior of the moon, taking with it a part of the surface ice that has been melted, and by a series of such ebbs and flows Mr. Peal conceives the terraced walls of the lunar craters to have been built up above the level of surrounding plains. Putting on one side the difficulty of conceiving of nearly perpendicular ice cliffs of 17,000 to 20,000 feet high, standing for ages without flowing down as glaciers to the plains at their feet, we have to account for the fact that the lunar plains and the floors of the deeper lunar craters are generally of a much darker tint than the higher ground upon the moon; while if the whole of the lunar surface were composed of ice and snow there would be no reason for such a difference of tint, unless, as Mr. Peal suggests, the lunar plains are surfaces of virgin ice while the mountains are formed of snow. But virgin ice would reflect the light of the sun specularly, and in the equatorial and tropical parts of the moon from which the sun's rays could be specularly reflected to us there are no traces of such specular reflection. The theory also fails to account for the small craters frequently found on the rims of larger craters and on the sloping sides of mountains; such small craters are far above the assumed rock surface of the moon and warm water issuing from them would flow down the sides of the mountains, leaving marked traces of its flow.



From a photograph of the Moon taken by the Brothers Henry on the 29th March, 1890.

The meteoric theory of the formation of lunar craters has also had many advocates. It is alleged that if a pebble be dropped into mud the scar produced has a raised rim and a central hill, which resembles a lunar crater. Even Mr. Proctor had an inclination for this theory. At page 346 of his book on the moon, he says:—"So far as the smaller craters are concerned, there is nothing incredible in the supposition that they were due to meteoric rain falling when the moon was in a plastic condition. Indeed, it is somewhat remarkable how strikingly certain parts of the moon resemble a surface which has been rained upon, while sufficiently plastic to receive the impressions, but not too soft to retain them. Nor is it any valid objection to this supposition, that the rings left by meteoric downfall would only be circular when the falling matter chanced to strike the moon's surface squarely; for it is far more probable that even when the surface was struck obliquely, and the opening first formed by the meteoric mass or cloud of bodies was therefore markedly elliptic, the plastic surface would close in round the place of impact until the impression actually formed had assumed a nearly circular shape." After inviting attention to the lunar photographs published with his book, Mr. Proctor continues:—"It will be seen that the multi-

mapped (Reclus) as 16 by 14 miles in extent; (2) the crater of Asosan, Isle of Kiushiu, Japan, is 15 miles across (Milne); (3) Scrope mentions a circular crateriform lake, about 15 miles in diameter, in Northern Kamschatka ("Volcanoes," second edition, London, 1862, p. 457); (4) an imperfect crater cirque on Mauritius, mentioned by Charles Darwin, is mapped (Admiralty) as about 15 by 16 miles in extent; (5) the crater walls surrounding Lake Bolesna, Italy, are mapped as 11 by 9 miles in extent; (6) the crater containing Lake Maninju, Sumatra, is mapped (Reclus) as 15 by 7 miles in extent.

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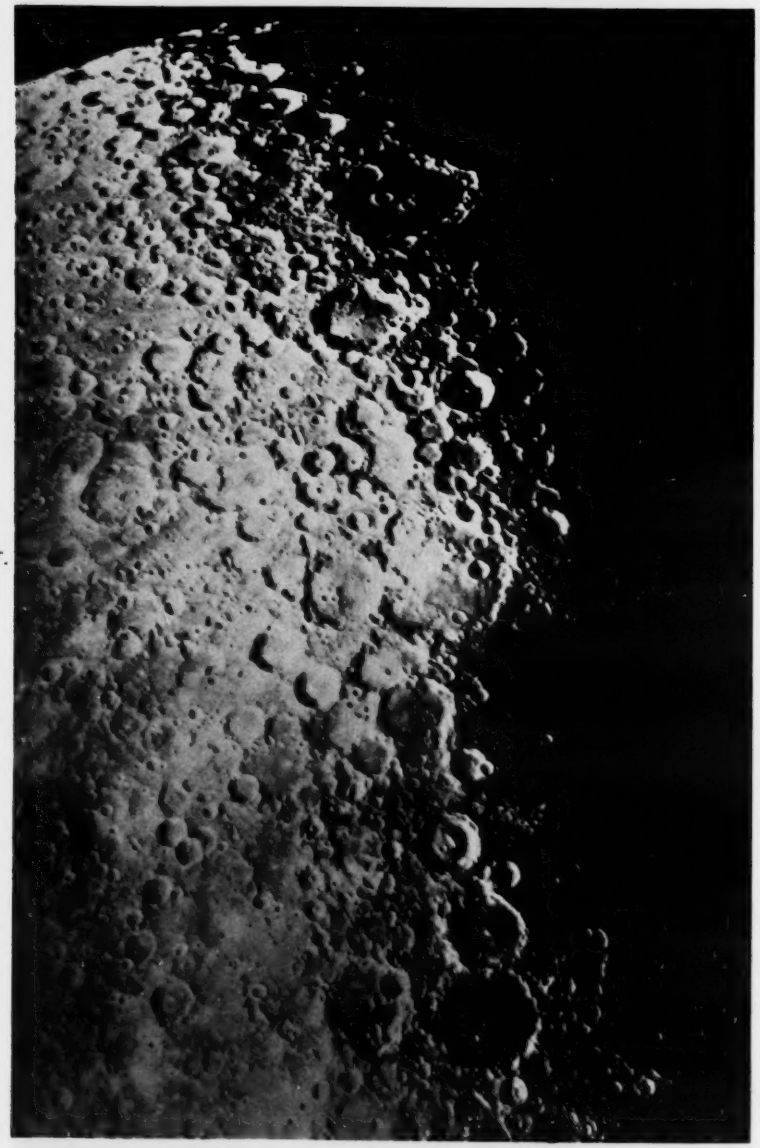

EAST.

PHOTOGRAPH OF THE MOON'S SOUTHERN POLE.

Taken when the Moon was 215 hours old, by MM. PAUL and PROSPER HENRY, at the Paris Observatory, on the 29th March, 1890.

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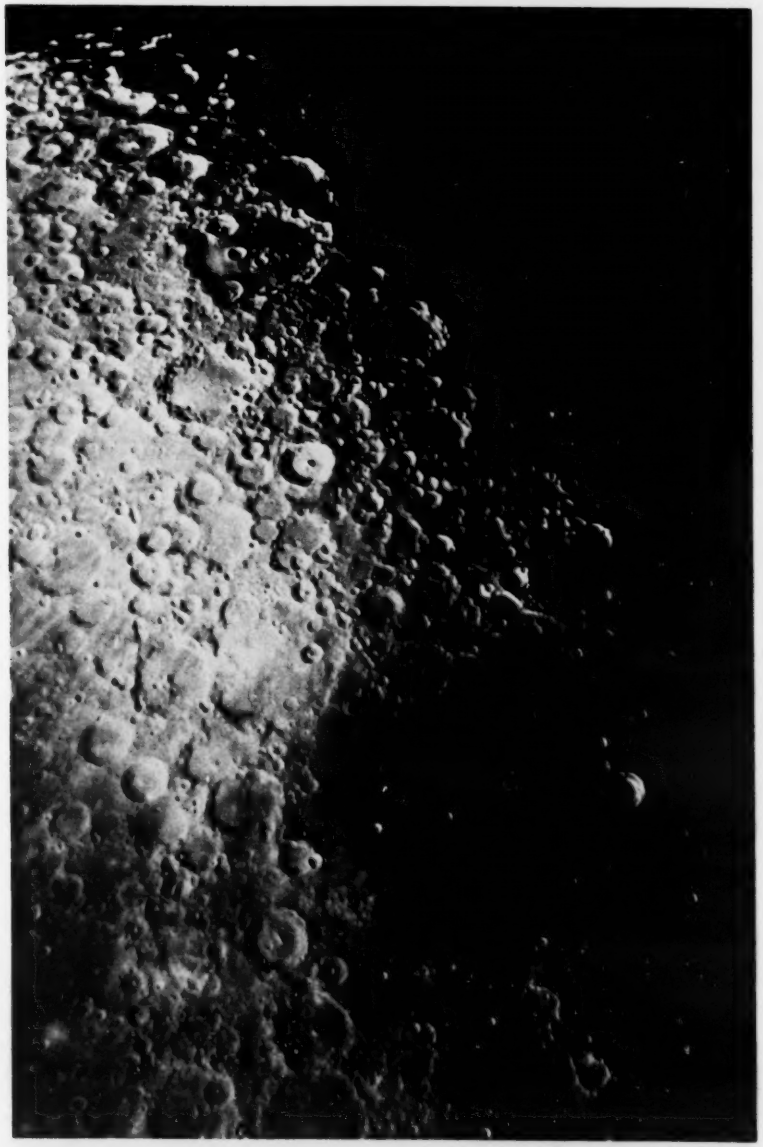

EAST.

PHOTOGRAPH SHOWING THE REGION TO THE WEST OF TYCHO.

Taken when the Moon was 240 hours old, by MM. PAUL and PROSPER HENRY, at the Paris Observatory, on the 28th May, 1890.

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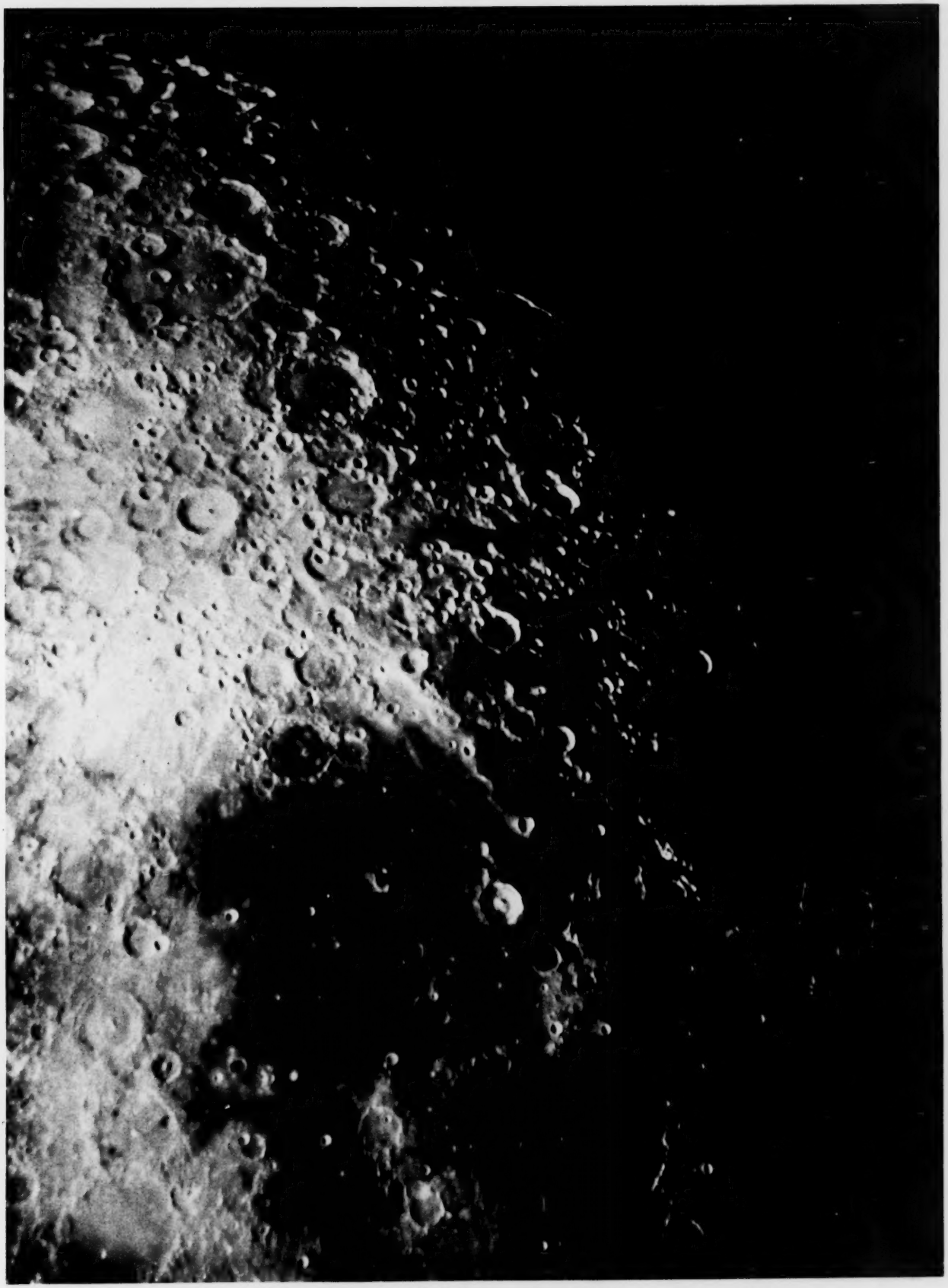

EAST.

PHOTOGRAPH OF THE REGION ABOUT THE LUNAR CRATER TYCHO.

Taken by MM. Paul and Prosper Henry, with the 13-inch refractor of the Paris Observatory, on the 29th May, 1890, the Moon being then 265 hours old. The sensitive plate was placed behind an eye-piece which enlarged the image in the principal focus fifteen times.

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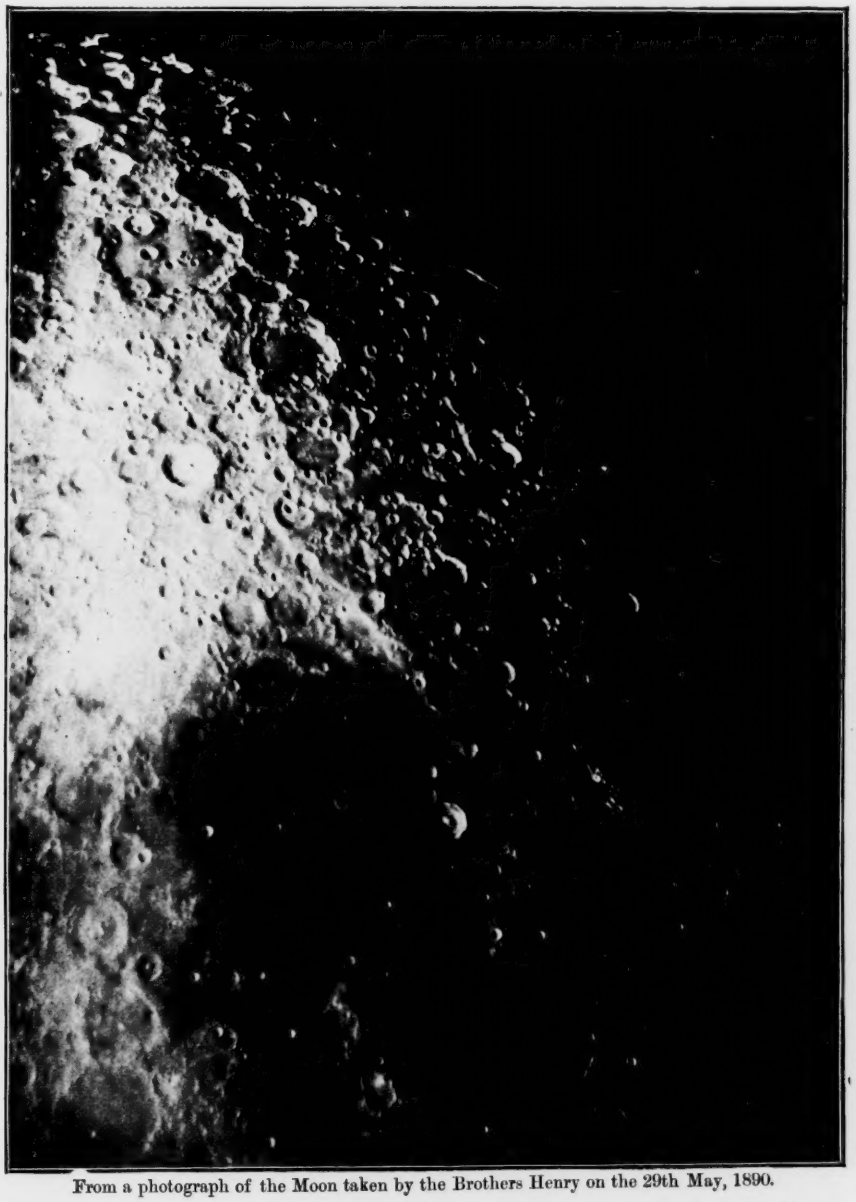
tudinous craters near the southern part of the moon are strongly suggestive of the kind of process I have referred to, and that, in fact, if one judged solely by appearances, one would be disposed to adopt somewhat confidently the theory that the moon had had her present surface contour chiefly formed by meteoric downfalls during the period of her existence when she was plastic to impressions from without. I am, however, sensible that the great craters, under close telescopic scrutiny, by no means correspond in appearance to what we should expect if *they* were formed by the downfall of great masses from without. The regular, and we may almost say battlemented, aspect of some of these craters, the level floor, and the central peaks so commonly recognized, seem altogether different from what we should expect if a mass fell from outer space upon the moon's surface. It is, indeed, just possible that under the tremendous heat generated by the downfall, a vast circular region of the moon's surface would be rendered liquid, and that in rapidly solidifying while still traversed by the ring waves resulting from the downfall, something like the present condition would result."



From a photograph of the Moon taken by the Brothers Henry on the 29th May, 1890.

More recently the meteoric theory of the formation of lunar craters has been taken up and considerably elaborated by an American, Mr. G. K. Gilbert, who has made the theory the subject of an address, delivered when retiring from the presidency of the Washington Philosophical Society, on December 10th, 1892. Recognizing the difficulty alluded to by Mr. Proctor, viz., that most of the lunar craters are circular, while if the meteoric bodies came from outer space many of them ought to strike the moon's surface very obliquely and produce elliptic rings, Mr. Gilbert made a series of experiments in the laboratory, and found that when projectiles were thrown obliquely against a target of plastic materials a crater-shaped hole of elliptic contour was formed. In order to obviate this objection to the theory, he assumes that the bombarding masses which gave rise to the lunar craters did not come from outer space, but were originally parts of a ring about the earth, similar to the ring which encircles the planet Saturn. From this ring he supposes that the moon was gradually formed, the small bodies constituting the ring having first coalesced into a large number of moonlets, which finally all united into a single sphere. According to this hypothesis the lunar craters are the scars produced by the collision of the moonlets which last surrendered their individuality; and, according to Mr. Gilbert and a mathematical friend who aided him in the investigation, 58 per cent. of the moonlets would under the circumstances imagined strike the surface of the moon, making an angle of less than 20° with the vertical, while 70 per cent. would strike at an angle of less than 30°, and 80 per cent. at an angle of less than 40°. From laboratory experiments, Mr. Gilbert found that the ellipticity of the scars on his plastic target increased slowly up to an incidence of 40° to the vertical, and that beyond that incidence the resulting scars

showed considerable ellipticity. He assumes that, owing to the flat character of the Saturnian ring about the earth, the moonlets must have approached the moon approximately in the plane of its equator; but the fact is not attested by the grouping of the craters in a medial zone. Mr. Gilbert therefore assumes that the axis of the moon's rotation has shifted under the successive impulses of the bombardment, and that the moon's equator has occupied successively all parts of its surface. He assumes that the velocity of impact due to the moon's gravity would be sufficient to melt the rocks of the lunar surface, and that they would during a short period behave as if they were composed of plastic material, but would become hardened before the crater could subside.

The theory does not at all commend itself to my mind. M. Roche, of Montpellier, showed that a ring about a planet would break up if it extended beyond a distance of $2\frac{1}{5}\frac{1}{5}$ths the radius from the centre of the planet; and if the density of the planet increased towards its centre, the maximum limit to which a ring could extend would be still further contracted. A moon formed just outside such a ring would have an ellipticity greater than that of an ordinary hen's egg; and as tidal action carried the moon away from its primary it would gradually approximate to a spherical form. One can hardly conceive that such a change of shape could take place without obliterating scars on its surface; but there is another objection to the theory, which, to my mind, is even more conclusive. There are upon the moon many lines or strings of small craterlets which fall very evidently into line with one another. If we are forced to treat them as scars upon a target, we must regard their alineation as the result of mere chance distribution; but the number of such strings precludes any such assumption; there must therefore be a physical reason for the alineation, and the most obvious assumption seems to be that the craterlets mark out a line of weakness in the crust of the moon and lie along a volcanic fissure or lunar fault.

Riphæan Mountains and the crater Euclides.

There is every gradation in size and in type from the small craterlets or cup-shaped depressions up to the gigantic walled rings, and any theory which professes to account for craterlets must account for the types of crater into which they gradually merge. We therefore seem driven back to the volcanic hypothesis, and have to explain why upon the moon, which is so much smaller than the earth, the volcanic outbreaks have been on so colossal a scale. We are not even in a position to say that the moon is made of similar materials to the earth—indeed, we know that its average density is considerably less, the earth being about 5·66 times as heavy as a similar globe of water, while the moon is only about 3·39 times as dense as water, or, according to Dr. Gill's recent determination, about one per cent. less. We must not, however, conclude from this difference that the moon is made of different materials from the earth, for we know too little as to the behaviour of solids under the enormous pressures that they must be subjected to at even a few miles beneath the earth's surface. The average density of the rocks of which the earth's surface is composed is only about two and a half times that of water, but it does not follow that the central parts of the earth are composed of different and heavier material. The great rigidity of the earth under the tidal strains imposed upon it by the sun and moon points to the conclusion that the solid materials of which the earth is built up are rendered rigid by compression, and that the idea of a fluid interior must be abandoned. Mr. George F. Becker, of the United States Geological Survey, has recently pointed out that the slags into which most of the stratified rocks of the earth's surface* would be reduced by melting, increase in bulk on fusion, and are not like iron and water, which expand on solidifying; consequently, he argues that any crust which formed on the surface of a molten sphere of slag would speedily break up by its own weight and sink, and that the process would go on until the whole mass had been reduced in temperature by such upheavals to near the melting point of the slag. But if the liquid slag, or other materials of which the earth is composed, were capable of being reduced by the pressure of the superincumbent mass to the solid condition, such upheavals would not take place, and under such circumstances it is possible that the heat of the earth may go on increasing to its centre.

If below the surface of the earth large masses of highly-heated rock are kept solid by the enormous pressure of overlying rocks, earth movements caused by the cooling and contraction which crumple up the stratified rocks of the surface and give rise to the upheaving of mountain chains, may occasionally take off some of the weight from the rocks beneath, causing the highly-heated rocks to run off into the liquid state again and find their way to the surface, causing the phenomena we know as volcanic action.

If we adopt this theory of volcanic action, and assume that the moon is made of similar materials to the earth, we shall with lunar gravity only equal to one-sixth of terrestrial gravity need to pass to a depth six times as great upon the moon in order to obtain the pressure necessary to solidify liquid lava at a temperature equivalent to that at which it is solidified beneath the earth's surface, and any change of pressure that releases a stratum of rock from the solid to the liquid state would upon the moon release a stratum approximately six times as thick, other conditions being similar, and would presumably give rise to lava flows on a gigantic scale compared with terrestrial evolutions. Added to these considerations, we must remember that under the feeble action of lunar gravity crater rings and cliffs may be built up of similar material much more steeply upon the moon than at the earth's surface.

Thebit

Birt

Straight Black Channel between the craters Birt and Thebit. From two photographs taken by the Brothers Henry.

There are many formations upon the moon which do not take the form of crater rings. The Riphæan Mountains, shown in our photograph of May, 1890, is a very good instance to cite. I should like to draw special attention to a curious straight black streak between the craters Birt and Thebit. It is spoken of by Webb as a wall, but it rather seems to be a narrow valley or fault. It is shown on several of the photographs taken by the Brothers Henry. I would also draw the reader's attention to three dark spots on the floor of the crater Alphonsus, which are shown in all good photographs, as well as the curious marking, like a capital G, near to the centre of the Mare Nubium.

* In an article on the subject by Mr. Becker in the *North American Review* for April, 1893, he states that granite and allied rocks increase in bulk on fusion by about ten per cent.

The imperfect skull of a simply enormous lemuroid animal, from south-west Madagascar, has been exhibited by Dr. Forsyth Major at the Zoological and Royal Societies. It is part of a find sent to the British Museum, and including also some very remarkable *Æpyornis* remains. The front portion of the mammalian skull is injured, and only the molar and premolar teeth (of a distinctly lemuroid type) are preserved. There is a high and outwardly projecting zygomatic arch, and the inter-orbital part of the frontals has enormous lateral developments above the orbits. All the bones of the skull are greatly thickened. The brain case is remarkably small, and the cranio-facial angle very obtuse. The remains were exhumed from a marsh covered by a stratum of shell marl about two feet in thickness.

Letters.

[The Editor does not hold himself responsible for the opinions or statements of correspondents.]

ON THE FORMATION OF CLOUDS IN THE ATMOSPHERE OF MARS.

To the Editor of KNOWLEDGE.

SIR,—It is generally supposed that the atmosphere of Mars is very rare, and we know from observation that it is generally clear and free from clouds. This has been attributed to the rarefied atmosphere being unable to contain much aqueous vapour, and it has also been concluded that the snow-line is very low, probably reaching the ground in all extra-tropical latitudes. It seems to me that these conclusions have been arrived at by straining an imperfect analogy too far, and that the observed facts can be explained in another, and it seems to me, more probable way. At a fixed temperature aqueous vapour can attain a certain maximum tension. When it does attain this tension, we speak of the air as being "saturated," suggesting an analogy between a salt dissolved in water and the aqueous vapour "dissolved" in the air; and also suggesting that if the *quantity* (apart from the volume) of this air was lessened, the amount of aqueous vapour necessary to saturate it would be lessened also. As a matter of fact it is not so. The maximum tension is quite independent of atmospheric pressure; at the same temperature it would be the same for a vacuum as for our air at sea-level. It is therefore a mistake to suppose that, because the atmosphere of Mars is rare, it cannot contain much aqueous vapour; volume for volume, it can contain exactly as much as our atmosphere could at the same temperature, whatever be its degree of rarity. The explanation of the observed facts must, then, be sought somewhere else.

Let us consider how clouds are formed on our earth. Water is evaporated at sea-level in the form of a transparent and invisible vapour, which rises rapidly, expanding and losing heat as it rises, till on attaining a certain level it has lost so much heat that it can no longer remain in the state of vapour; a considerable portion condenses into the liquid form, forming our lower or cumulus clouds. This level may be called the level of watery condensation. The portion of aqueous vapour which has escaped condensation continues to rise, and as it rises to expand and lose heat, till a higher level is reached at which it can no longer remain in the gaseous state. It then in its turn condenses, but its temperature is so low that it condenses in the solid form, forming our cirrus clouds. This level may be called the level of ice precipitation. I say ice and not snow, because the snow-level proper is a very different thing, and much lower. There is no snow-level in the air; it is confined to the solid surface, generally mountains, but coming down to sea-level in high latitudes. Probably the aqueous water, if rapidly forced up, as by ascending currents of air in blowing over a mountain slope, is, to a great extent, forced past the level of watery condensation, and when condensation does occur, a good deal of the aqueous vapour passes at once into the solid form, and is deposited as snow. Now let us consider how all this would be modified in the case of Mars, and for the present we will leave out of consideration the greater distance of Mars from the sun, and consider it as if it were at the same distance as the earth, or, at any rate, had the same temperature at the surface. Although the density of the Martial atmosphere is much less than ours, there would be just as much evaporation from an equal area of water surface as with us. Perhaps there would be even more, because evaporation would go on more readily into a rare atmosphere than into a dense one, although ultimately the quantities taken up would be equal. Now this aqueous vapour would rise on Mars as it does with us, but much more slowly, as the density of the Martial atmosphere diminishes much more gradually. This is not conjectural; it can be *proved* that, owing to the diminished force of gravity at the surface of Mars, the atmospheric density, which is halved at a height of three and a half miles with us, would only be halved there at a height of about nine miles. Moreover, the aqueous vapour would not only rise more slowly, but would attain a much greater height before precipitation would occur. If we suppose that on earth it occurs when the vapour has expanded to twice its original volume—that is, at a height of three and a half miles—an equal expansion would only occur on Mars at a height of nine miles. As a matter of fact, precipitation would not occur even then, because the aqueous vapour absorbs a certain amount of heat, partly by stopping the direct solar radiation, partly, and to a much greater extent, by intercepting the reflected radiation from the body of the planet. On the earth, where the vapour rises rapidly, and to a height of only three and a half miles, the amount so absorbed may be inconsiderable, but on Mars, where the vapour rises much more slowly, and to a height of nine miles, the amount absorbed must be much greater, and would raise the level of watery precipitation still further, say to a height of eleven miles. Hence the great height of the Martial clouds, and the slowness with which the vapour rises. We might expect that instead of our dense cumulus clouds, the prevailing cloud in the Martial tropics would be a high and thin stratus, covering nearly the entire sky. Now, we know from experience that a thin veil of stratus cloud interferes but little with the definition of objects seen directly through it—in fact, not unfrequently it improves definition; so that we might expect the centre of the disc would be clearly visible, and as we would have no unclouded portion for comparison, we might not suspect the existence of clouds at all. On the other hand, near the edge of the disc, the light from below coming very obliquely, would be almost entirely intercepted, while the clouds themselves would reflect a diffused white light. Consequently we might *a priori* expect that definition would be good in the centre of the disc, while the edges were surrounded with a whitish margin. Of course, the greater distance of Mars from the sun must modify this to some extent, but the close agreement between our theoretical deductions and the actual results of observation makes me think that the modification can only extend to altering details, leaving the general features the same.

XUM

The intensity of the solar heat received by Mars is, we know, only about two-fifths of that received by the earth; but if the earth only received two-fifths of the heat which it does now, the temperature would be so low that the seas would be completely frozen. Mars is probably considerably colder than the earth, especially at night, but it does not appear to be as cold as the distance of Mars would lead us to expect. There must, then, be some cause in the circumstances of the planet itself, preventing it from getting as cold as the earth would under similar circumstances.

The only causes of heat that appear to be possible are (*a*) internal heat, and (*b*) a peculiar constitution of the atmosphere. It does not seem likely that the internal heat of Mars can exceed (or even equal) that of the earth, so that we are almost compelled to conclude that the atmosphere is the cause. We know that a superabundance of aqueous vapour in the atmosphere would produce the effect; and we cannot suggest any other cause which appears reasonably probable. But if it be due to aqueous vapour, the atmosphere must be much damper than ours, because ours would not produce the effect, or anything like it.

Yours faithfully,

Dublin. J. R. Holt.

[The precipitation of aqueous vapour into cloud in the earth's atmosphere is evidently dependent on other conditions besides temperature, or clouds would form and spread over the heavens every evening as the sun sets. Recent investigations seem to show that the dust-laden condition of our atmosphere is intimately connected with the formation of clouds, and it seems probable from *a priori* considerations that the atmosphere of Mars is less dust-laden than the atmosphere of our earth. Other conditions remaining unchanged, the amount of dust in the atmosphere is dependent on the velocity of the wind, and the feeble gravity at the Martial surface would, as Mr. Maunder pointed out in his article on "The Climate of Mars," published in Knowledge for September, 1892, lead us to conclude that the Martial winds blow more feebly than terrestrial winds, for all winds have their origin in rising currents of heated air. If the atmosphere of Mars were as dense and vapour-laden as our own, we should expect to see sunset tints on the limb of Mars when gibbous; but no such tints are traceable, and we can hardly expect that the atmosphere of Mars is damper than our own, for as Mr. Holt correctly points out, the amount of aqueous vapour in the Martial atmosphere must depend upon the temperature of the Martial surface, and not on the density of the Martial atmosphere. I am therefore inclined to conclude that the surface of Mars is much colder than the earth's surface, and that the white material which is deposited at the Martial poles, when turned away from the sun, and which melts or disappears as the poles turn towards the sun, cannot be snow, but is more probably some vapour which is precipitated as snow at a far lower temperature than 32° Fahr. Carbonic acid gas when frozen forms a very white, snow-like substance, and an atmosphere similar to our own, or any other mixture of gases forming an atmosphere about the planet, would, if solidified into small crystals, appear white and snow-like.—A. C. Ranyard.]

To the Editor of Knowledge.

Dear Sir,—The question raised by Mr. Holt in your last number is well worthy of consideration. The Greek word corresponding to the Latin "focus" is ἑστία, but as that word has never, I believe, been applied in a mathematical or astronomical sense, I fear "periestia" would hardly be an appropriate word to express the point in the orbit of a satellite which is nearest to its primary. The objection to "pericentron" is that its etymological meaning would be ambiguous. If I might venture on a small pleasantry (to be taken in a courteous sense), I would kick against that suggestion, as reminding one of a Greek proverb about an ox-goad.

Independently of "focus" being a Latin word, in the mathematical sense every ellipse has two foci, one towards each apse; so that if there were a Greek word exactly corresponding, the prefixing "peri" to it would not make it clear whether the point nearest to or farthest from the primary planet were intended. Now for these two points collectively we have already the word "apsides"; and if it be desired to coin a word to distinguish that which is nearest the centre of force, perhaps "protapsis" might do as the principal or most important apse, whilst "hyperapsis" (upper apse) would do for the other—the two corresponding to perihelion and aphelion in the orbit of a primary planet. As Montucla points out, the word "focus" was introduced into the conic sections by modern mathematicians from its optical signification; the ancient geometers designated the point by the Latin expression, *punctum comparationis*, and its Greek equivalent.

Mr. Holt alludes to the *hybrid* word "perijove," which has obtained some currency and is decidedly objectionable, though probably the correct form could never be adopted. Unfortunately, there are many instances of want of sufficient care in the formation of words from the Greek. In a conversation I had recently with Prof. Barnard on the fifth satellite of Jupiter, he informed me that many persons in California (with whom he did not himself agree) wished him to name it Eureka, the word being on the seal of that state. There are obvious objections to this application of the word; but it is unfortunate that it has been adopted into English in an erroneous form, for it certainly should have an aspirate and be spelt Heureka, as doubtless pronounced by Archimedes when fresh from his bath.

Blackheath. Yours faithfully,

July 18th, 1893. W. T. Lynn.

THE SPECTRA AND PROPER MOTIONS OF STARS.

To the Editor of Knowledge.

Sir,—As I am just leaving on a vacation trip, during which I shall be away from books and documents, I must defer for the present any remarks on the letters of your correspondents Prof. Kapteyn and Mr. Boraston. We undoubtedly require more research before arriving at definite results, and the extension of the Draper Catalogue to the southern hemisphere, when completed, will furnish very important materials. I may mention that, suspecting that many faint stars with considerable proper motion described as Sirian in the Draper Catalogue were really solar, I wrote to Prof. Pickering, who kindly directed a re-examination of the spectra of the stars mentioned in my letter, and has lately sent me the result. A very considerable proportion of these stars turn out to be really solar stars, mainly of the Capellan type. I made some further inquiries with respect to Capellan stars whose proper motion appeared to be smaller than I expected, but in these instances the new examination led, in the great majority of cases, to the same result as the old. The principal exception is Rigel or β Orionis, whose spectrum is not F but B—this spectrum, according to my view, denoting the most brilliant, and therefore the most distant (relatively to their magnitudes), of all stars. The spectrum of 1830 Groombridge is G.

Truly yours,

16, Earlsfort Terrace, Dublin, W. H. S. Monck.

July 5th.

Notices of Books.

Moral Teachings of Science. By Arabella B. Buckley. (Edward Stanford.)—Unlike most of Miss Buckley's works, the present little book is not specially addressed to young people, but to those who, as she says in her preface, "feeling puzzled and adrift in the present chaos of opinion, may welcome even a partial solution, from a scientific point of view, of the difficulties which oppress their minds." It is an earnest and sober attempt to throw the light of optimism upon some of the most serious of the problems which beset the thoughtful and sympathetic student of human life. Starting with an exposition, accompanied by illustrative details, of the grand generalizations which have been amongst the proudest achievements of modern science, she endeavours to show that they tend in the direction of hope rather than of despair, even when their operation seems most ruthless; and thus she claims that the great facts of evolution, natural selection, and the uniformity of law have a bearing upon the sphere of morals and duty, and that they tend to give the advantage in the long run to right rather than to might. There rings through the book a healthy tone, which is likely to be beneficial and to make it helpful in building up a sturdy and robust habit of thought.

Happy Hours. By Martha Hill and Friends. (George Bell and Sons.)—An old friend in a new dress. The six volumes of the "Parents' Cabinet," originally projected by Mrs. Hill, have been re-issued with the above title, under the editorship of Miss Constance Hill, daughter of the original foundress. They comprise a collection of short pieces on all imaginable subjects interesting to children. Brief articles on natural history, scientific inventions, biography, &c., are judiciously interspersed with juvenile tales of a more general character; and all are written in a bright, pleasant, and more or less conversational style, which will be as welcome to the rising generation of to-day as it was to their predecessors of forty years ago. The scientific articles are generally accurate and up to date, as well as suited to the comprehension of the auditory for whom they are composed. In these volumes, each of which is complete in itself, mothers will find a useful addition to the nursery library, containing as they do matter suited to varied ages and tastes.

The Apodidæ: a Morphological Study. By H. M. Bernard, M.A. (Macmillan & Co.)—This is a remarkable book. To none but experts will the title be likely to convey much information, for the Apodidæ are not a very widely known set of animals. They are a numerically small group of fresh-water crustaceans, constituting a family of the order Phyllopoda. The specimens upon which Mr. Bernard's work was done were chiefly received from the Arctic regions, and especially from Spitzbergen. It is much more than a mere detailed account of the morphology of this curious group; it is the laborious investigation of a thorough-going phylogeny-hunter, carried out with zeal and energy under the inspiration of a bold and original speculation, and the problem whose solution is proposed gradually grows from that of the origin of this particular group to so large a generalization as the genealogy of the whole class Crustacea. The theory the author endeavours to establish is primarily that the Apodidæ are derivable from an annelid or primitive worm that adopted a certain peculiar habit of life, and that in fact they represent the original stock of the Crustacea and constitute a transitional form between that class and the Annelida. His argument proceeds upon the assumption that a primitive carnivorous worm exchanged the habit of capturing its prey by the protrusion of its pharynx for a browsing method of feeding, and that "the further development of this habit would lead to a *bending round* of the head sufficient to enable the animal to use its anterior parapodia for pushing prey into its mouth; in time the bend of the head would become fixed, and the parapodia modified as jaws and maxillæ." The argument depends entirely upon this assumption of the *bending* of the first five segments of the worm's body, and one would have expected the author therefore to be a little more anxious to show the reasonableness of his preliminary hypothesis. It is not easy to see what should have tempted this primitive worm to adopt as an established habit a method of taking its food which seems as though it must at first have been extremely awkward; and yet the results of such adoption were not, according to the author, either insignificant or temporary, but of the most far-reaching consequence, for from this bent annelid he proposes to derive the innumerable forms of both existing and extinct Crustacea. Apart from this initial difficulty the argument is conducted with much ingenuity, though occasionally the author appears to allow his enthusiasm to outrun his caution, and he is surely venturing on risky ground when he says, speaking of the possibility of deducing the various crustacean organs, one by one, from structures known to exist in the Annelida, "it is enough for our argument if we can show that such a deduction is *possible:* it is not essential to our theory that we should show exactly *how* the inner transformation actually took place." Such a statement seems to need a more comprehensive qualification than is contained in what follows, that "the validity of our argument can only be seriously weakened by showing . . . that the improbability of such a transformation is so great that no experienced morphologist would accept it."

THE MIGRATION OF BIRDS.

By G. W. Bulman, M.A., B.Sc.

In his book on "The Migration of Birds,"* Mr. Dixon has focused the scattered rays of knowledge we possess on this subject. For the first time, he claims to have gathered together in one volume an outline of the whole science of the subject. The book commences with a sketch of the history of ancient opinions on the migration of birds, and passes on to a discussion of hibernation. On this question Mr. Dixon is careful not to commit himself to any decided opinion. It is, indeed, a delicate subject to handle, and it is hardly possible for an ornithologist to touch the pitch of the hibernation theory without being defiled; or as it has been said, "it is as much as a virtuous ornithologist's name is worth for him so much as to whisper hibernation, torpidity, and mud."

In spite of this danger, and "at the risk of being 'handled without gloves' by some mud and torpor despising bruiser critic," Mr. Dixon expresses his conclusion as to hibernation thus:—

"I neither accept nor deny it, although fully believing it possible, considering that such an attitude is the most scientific position to assume until the subject has been more fully investigated."

From the connection in which the name of Olaus Magnus is mentioned, a careless reader might imagine he was a contemporary of Aristotle. Mr. Dixon might have mentioned that he was a Swedish divine, who lived in the sixteenth century, and wrote on the natural history of his country. Olaus Magnus was acquainted with the idea held by others, that swallows migrated to warmer countries for the winter, but expressed his own opinion thus:—

* "The Migration of Birds," by Charles Dixon. London, 1892.

"In the beginning of autumn they assemble together among the reeds, where, allowing themselves to sink into the water, they join bill to bill, wing to wing, foot to foot."

Many recorded cases of hibernation are quoted by Mr. Dixon from various authorities, and they are on the whole difficult to explain otherwise. Yet he brings forward nothing as the result of his own observation, and this, be it remarked, is usually the case in the literature of hibernation. A more recent example than any of those given by Mr. Dixon is one recorded in a letter written by an observer, Prof. Carlo Spegazzini, living in the Argentine Republic, published in *Nature*, July 4th, 1889. Writing to a friend in Italy, he says:—

"The bird known here by the name of Golondrina, and which I think is *Progne domestica*, is subject to hibernation. Last year, while the zinc roof of a small house was being taken up in the month of August, just in the middle of our winter, I found underneath about a hundred martins, all accumulated one over the other and lethargic, but in good health, so that, exposed to the sun, they awoke and flew away very briskly. This year, again, having seen some holes on a barrancha, a steep bank over the Plata, I began to dig at them, hoping to get some bats; but there I found several hundreds of martins, of the same kind as above mentioned, clustered and in a state of lethargy. Is such a thing known to naturalists?"

"For upwards of 250 years," says Mr. Dixon, "the hibernation of birds has more or less excited the curiosity of man." But it was a familiar idea more than 1400 years ago, as the following lines from the Roman poet Claudian (A.D. 408) show:—

> "Vel qualis gelidis plumâ labente pruinis
> Arboris immoritur trunco brumalis hirundo."

And 400 years earlier still, Pedo Albinovanus, a friend of Ovid, also alluded to the hibernation of the swallow:—

> "Conglaciantur aquæ, scopulis se condit hirundo
> Verberat egelidos garrula vere lacus."

The old state of doubt as to what became of the migratory birds is expressed in Pope's lines:—

> "A bird of passage, lost as soon as found;
> Now in the moon perhaps, now underground."

And Mr. Dixon reminds us, that with all our recent advance in the science of migration, the winter home of two of our common summer migrants, the house martin and the sand martin, is still practically unknown.

One of the most interesting results established by the researches of the British Association Committee is that, with rare exceptions, the young of the year migrate before the old. This seems to preclude the idea held by many, that the young learn the route by experience, being led over it in the first case by the old. But Mr. Dixon tells us, that while the main body of the young do migrate before the main body of the old, yet the first to take the field are certain individuals which have not been able to breed, or have lost their eggs or young. These old birds act as *avant-courières* of the migratory host, and guide the inexperienced young birds. Another fact which may be thought to lessen the wonder of the young finding their way is, that large numbers of them are lost. The mortality during migration is enormous, and according to Mr. Dixon is largely due to young birds *losing their way*. Taken together, these two facts may be accepted by many as sufficient explanation of the young birds finding their way when migrating before the old; in my opinion, however, they do not go far enough. In the case of the first, supposing it firmly established that a few old birds do take the lead, can we suppose there would be a sufficient number of them to act as guides to the young over large districts? A few here and there would hardly suffice. And the absence of breeding cares can hardly be considered a sufficient cause for the migration of these stray individuals, when we remember that the *main body* of the old birds remain behind after such cares have ceased, and even after the young have left the country. Breeding cares, again, are not *in themselves* sufficient to keep the old birds in the country, as we know from the case of certain individuals breeding exceptionally late, which have actually left their young in the nest in order to migrate. In other words, they will stay after breeding cares have ceased, and breeding cares are not sufficient to keep them after a certain time. As to the second, it is perhaps impossible to prove that the mortality is really greater among young birds through losing their way than among the old.

The marvellous way in which birds find their way across thousands of miles of land and sea in their annual migrations has long excited the wonder of naturalists. Dr. Von Middendorff suggested that birds might be endowed with something which rendered them sensitive to the magnetism of the earth, so that they were able to adjust their course as a mariner does his by the magnetic compass.

Prof. Weismann, in an essay on migration, considered the faculty to be due to *experience*, not of the *individual* but of the *race*. In other words, he believed the sharpened faculties of one generation were transmitted by heredity to the next; a conclusion upon which his own recent investigations on acquired characters throw doubt.

According to Mr. Dixon, the "mysterious sense of direction" with which many writers have gifted migratory birds is a "myth." Their power of finding their way, he thinks, is due to their keen sight, and the fact that they usually fly sufficiently high to take a bird's-eye view of a great part of their route at once. And in crossing the sea, in which the distance passed over is usually not more than 300 miles, it is supposed that the height at which they fly enables them to take in the land-marks on the opposite shore. This power of sight, added to a very retentive memory for land-marks, is thought to be sufficient to account for their power of finding their way. In regard to the question as to the height at which migrating birds usually fly, Mr. Dixon suggests the use of a captive balloon from which to make observations.

There are four classes of lines of migration, viz.:—sea routes, coast routes, river routes, and mountain routes. Of these, the first two are of special interest, as suggesting old vanished coast lines and land connections. Birds in their migrations seem usually to choose those lines which present the least expanse of water to be crossed, and the routes by which our summer visitors reach us and return are by the Straits of Gibraltar, Corsica and Sardinia, and Sicily and Italy; and these routes correspond with the old land connections between Europe and Africa, as seen, for example, in Prof. J. Geikie's map of prehistoric Europe. It is supposed by many that the passage of the migratory hosts by these particular lines is due to the conservatism of a habit practised by their remote ancestors when the present water-way was land. As the land gradually sank, successive generations of birds passed over the gradually increasing distance of sea without experiencing much lengthening of the journey. In this way, the studies of the geologist and the ornithologist confirm each other.

It is to be remarked, however, that if, as Mr. Dixon believes, birds fly at a sufficient height to take in at a glance vast extents of country, we may well suppose that individual birds at the present day may, from such a view, be enabled to *choose for themselves* the shortest route without any aid from inherited habits.

Perhaps the most interesting indication of an old land surface is that suggested by the migration of the orange-legged hobby (*Falco amurensis*). Breeding in Eastern Siberia, Mongolia, and North China, it migrates to winter quarters in India and South-East Africa. Its suggested route is by the Maldive Islands, the Chagos Archipelago, and the Saya de Malha Banks, and in this passage it is supposed to be following an ancestral habit acquired when India was connected with Africa, and the migration could be performed by land.

Such an Indo-African continent has also been suggested by the geologist from the study of the rocks of the two areas, and by the zoologist from a study of their faunas.

In the Indian rocks, forming what is known as the Gondwana system, an assemblage of organic remains are met with, presenting such a remarkable similarity to those of South Africa and Australia that Dr. Blandford has been led to advocate the existence of an expanse of land connecting India with these continents during the Gondwana period. And naturalists studying the present faunas, and finding a striking similarity between that of India and that of Madagascar and the adjacent islands, likewise require an Indo-African continent to explain their facts. The name Lemuria has been proposed for this continent, since it is supposed to have been the original home of the family Lemuridæ, now divided by the Indian Ocean into the two separate regions of Madagascar and South Africa on the one hand, and South Asia on the other. It is to be remarked, however, that the migration of the orange-legged hobby, and the distribution of the Lemuridæ, require a *much more recent* land connection than the ancient Gondwana continent of Dr. Blandford.

Coast migration, again, has suggested the existence of former coast lines. Thus the knot, the bar-tailed godwit, and the grey plover migrate up the east coast of England as far north as Spurn Point, and then strike across the sea eastwards. They are supposed to be following an old coast line once reaching from Spurn Point to Denmark or Holland; and geologists have certainly given us a restored Britain, in which our eastern sea-board was united to the Continent. Yet the northern coast of this uniting land was far away to the north of Spurn Point. Still, if we suppose the submergence of the land was gradual, and from the south northwards, there may have been a time when the coast line, after trending north to Spurn Head, turned eastward to the Continent. And if this phase in the physical geography of the land was a comparatively permanent one, the peculiar line of migration followed by the grey plover might have been learned. It seems a more reasonable supposition, however, that birds which had learned to follow a *coast line* would continue to follow a *gradually altering* coast line, rather than take to the sea.

It is interesting to note that birds, in migrating over continental areas, make large use of valleys and water-courses, and less frequently of mountain ranges and passes. And just as many of the species inhabiting warmer regions retire north to breed, so others ascend the mountains for the same purpose. Thus in India, for example, birds retire to the hill stations for coolness, just as do the human inhabitants.

Like other writers on the subject, Mr. Dixon attributes the origin of migration to lack of food and unsuitableness of temperature; and he considers that these causes have been largely brought into operation by the climatal changes of the glacial period. Birds, he considers, have only been subject to one glacial period, for he rejects the evidence for any of them between that known as the Great Ice Age and Permian times. Migration, as we see it now, commenced with the glacial epoch. Mr. Dixon illustrates the initiation of migration by taking the spotted fly-catcher as an example. During a mild period it was a resident species in one unbroken area from the Arctic woodlands to the Pacific. As the cold period came on, the spotted fly-catcher was driven further and further south. Then they began to leave their northern haunts in autumn, and gradually, as the cold increased, their journey was lengthened. As glaciation ceased, they began to move north again, and also to journey north in spring to breed, returning south in winter, as they had learned during glaciation. Migration, then, consisted at first of very small journeys, gradually increasing as the ages rolled on.

In relation to the ornithological axiom, that "the birds which go the furthest north to breed go the furthest south to winter," Mr. Dixon expresses the belief that the habit indicates an ancient migration from pole to pole. Many birds which breed in the Arctic regions extend their winter migration far south of the Equator, as the sanderling, which reaches the Malay Archipelago, Cape Colony, and Patagonia; and the curlew sandpiper, which flies as far south as Australia for the winter; and quite possibly some species may even yet perform the journey from pole to pole. Thus Mr. Hudson, in "The Naturalist in La Plata," mentions certain species which come from the Arctic regions to winter on the Pampas, and of which species individuals also reach the same area from Antarctic regions. May not some of these be birds which have actually passed from the Arctic to the Antarctic regions? And Mr. Dixon suggests that these flights for vast distances southwards are due to inherited love of home; for he believes that during the glaciation of the northern hemisphere the species were transferred from the north polar basin to the more genial south; and now, when their home and breeding place is in the milder northern regions, they show their hankering after the old home by flying as far south during the northern winter as circumstances permit.

Emigration, as distinct from migration, refers to the colonizing movements of birds by which they permanently leave their old haunts. And Mr. Dixon considers that the emigration of birds has had a vast influence on the origin of species. It leads to isolation of a number of individuals from the main body of the species, and the absence of intercrossing with the latter is supposed to be sufficient to allow of their developing into a new variety, and finally *species*, under their new and slightly different conditions.

Besides the great north and south migrations, by which our winter and summer visitors come and go, great east and west migrations in spring and autumn have been established. And, strange to say, the species moving in these latter are to a very large extent such as have always been looked upon as residents. Starlings, larks, sparrows, buntings, and many others leave Britain in immense numbers in autumn, and their place is taken by similar numbers of the same species from the Continent. In the spring the two streams are reversed, the one returning to Britain from the Continent, and the other passing back to the Continent from Britain. Almost all our native birds take part, more or less, in these migrations.

"With very few exceptions," runs the report of the British Association Committee, "the vast majority of our British birds, such as are generally considered habitual residents—the young invariably, the old intermittingly—leave these islands in the autumn, their place being taken by others, not necessarily of the same species, coming from more northern latitudes, or from districts of Eastern Europe. . . . These immigrants on the approach of spring leave, moving back to the Continent on the same lines,

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but in the reverse direction . . . . at the same time also our own birds return from the Continent to their nesting quarters in these islands."

The reason of such migratory movements can scarcely be explained as the result of lack of food and unsuitability of climate, since multitudes of the same species as those which leave pass the winter in the same country.

An interesting and apparently inexplicable fact recorded by the British Association Committee, and not alluded to by Mr. Dixon, is a migration westwards from the west coast of Ireland. Thus, in their report for 1887, they write :—

"At Rathlin O'Birne (West Donegal) immense flocks of birds—starlings, thrushes, and fieldfares—passed *west* from December 18th to 23rd. The nearest land to the west of this rocky island is America. This is not an isolated occurrence. The westerly flight of land birds at stations off the west coast of Ireland has been noticed on other occasions; the movement is apparently as reckless as that of the lemmings."

Certain naturalists have suggested, in all seriousness, that the westward migration of the lemming is in search of the lost Atlantis, to which their ancestors were wont to roam. Will they accept this westward migration of birds as a further illustration of the former existence of the Platonic continent?

THE FACE OF THE SKY FOR AUGUST.

By Herbert Sadler, F.R.A.S.

NEITHER sunspots nor faculæ show very perceptible signs of decrease. Conveniently observable minima of Algol occur at 11h. 39m. P.M. on the 11th, and at 8h. 28m. P.M. on the 14th.

Mercury is too near the Sun during the first portion of the month to be visible, he being in superior conjunction on the 8th. On the 20th he rises at 3h. 31m. A.M., or 1h. 23m. before the Sun, with a northern declination of 15° 39′, and an apparent diameter of $8\frac{1}{2}''$, $\frac{23}{100}$ths of the disc being illuminated. On the 25th he rises at 3h. 22m. A.M., or 1h. 40m. before the Sun, with a northern declination of 16° 1′, and an apparent diameter of $7\frac{1}{4}''$, $\frac{41}{100}$ths of the disc being illuminated. The next morning he is at his greatest western elongation ($18\frac{1}{2}°$). On the 31st he rises at 3h. 35m. A.M., or 1h. 38m. before the Sun, with a northern declination of 14° 47′, and an apparent diameter of $6\frac{1}{4}''$, $\frac{56}{100}$ths of the disc being illuminated. While visible, Mercury passes through a portion of Cancer into Leo, without approaching any conspicuous star.

Venus is an evening star, setting on the 1st at 8h. 33m. P.M., or 59m. after the Sun, with a northern declination of 11° 27′, and an apparent diameter of 11″, $\frac{9}{10}$ths of the disc being illuminated. On the 18th she sets at 8h. 13m. P.M., with a northern declination of 3° 8′, and an apparent diameter of $11\frac{3}{4}''$, $\frac{87}{100}$ths of the disc being illuminated. On the 31st she sets at 7h. 45m. P.M., or 57m. after the Sun, with a southern declination of 3° 34′, and an apparent diameter of $12\frac{1}{2}''$, $\frac{84}{100}$ths of the disc being illuminated. Her brightness at the beginning of the month is about one quarter of what it will be at the beginning of next January. During August she passes from Leo into Virgo, being near the $4\frac{1}{2}$ magnitude star χ Leonis on the 8th, and the 4th magnitude star σ Leonis on the 12th. On the evenings of the 18th and 19th she is not far from the $3\frac{1}{2}$ magnitude star β Virginis, and on the 25th she is near the 4th magnitude star η Virginis, and at the end of the month to the south-west of Saturn.

Mars is invisible; and as Neptune does not rise till after midnight at the beginning of the month we defer an ephemeris of him till September.

Jupiter rises after 11h. P.M. at the beginning of August, and is getting into a favourable position for observation. He rises on the 1st at 11h. 15m. P.M., with a northern declination of 18° 41′, and an apparent equatorial diameter of 37·5″, the phase on the *p* limb being perceptible. On the 19th he rises at 10h. 9m. P.M., with a northern declination of 19° 8′, and an apparent equatorial diameter of 39·4″. On the 31st he rises at 9h. 25m. P.M., with a northern declination of 19° 19′, and an apparent equatorial diameter of 40·8″, the phase on the *p* limb amounting to 0·4″. He is in quadrature with the Sun on the 23rd. During the month he describes a short direct path in Taurus, being about 4° south of the Pleiades on the 1st, but without approaching any naked eye star during the month. At half an hour after midnight on the 29th a 9·5 magnitude star will pass behind the centre of the planet a little to the north, the occultation lasting about four hours. The following phenomena of the satellites occur while Jupiter is more than 8° above and the Sun 8° below the horizon :—On the 2nd a transit egress of the shadow of the second satellite at 1h. 5m. A.M., and of the satellite at 1h. 26m. A.M. On the 6th a transit ingress of the shadow of the first satellite at 2h. 25m. A.M., and of the shadow of the third satellite at 3h. 12m. A.M. An occultation reappearance of the first satellite at 3h. 6m. A.M. on the 7th. A transit egress of the first satellite at 0h. 26m. A.M. on the 8th. A transit ingress of the shadow of the second satellite at 1h. 19m. A.M. on the 9th. An occultation reappearance of the third satellite at 0h. 6m. A.M. on the 10th. An occultation reappearance of the second satellite at 1h. 29m. A.M. on the 11th. An eclipse disappearance of the first satellite at 1h. 29m. 31s. on the 14th. On the 15th a transit ingress of the first satellite at 0h. 10m. A.M.; a transit egress of its shadow at 0h. 59m. A.M.; a transit egress of the satellite at 2h. 21m. A.M.; an occultation reappearance of the first satellite at 11h. 30m. P.M. An occultation disappearance of the third satellite at 2h. 40m. A.M. on the 17th. On the 18th an eclipse reappearance of the second satellite at 1h. 17m. 56s. A.M., and its occultation disappearance at 1h. 49m. A.M. On the 21st an eclipse disappearance of the first satellite at 3h. 23m. 16s. A.M. On the 22nd an ingress of the shadow of the first satellite at 0h. 41m. A.M., a transit ingress of the satellite at 2h. 4m. A.M., and a transit egress of its shadow at 2h. 53m. A.M. An occultation reappearance of the first satellite at 1h. 24m. A.M. on the 23rd. On the 24th an eclipse disappearance of the third satellite at 1h. 6m. 4s. A.M., and its eclipse reappearance at 2h. 34m. 10s. A.M. On the 25th an eclipse disappearance of the second satellite at 1h. 39m. 41s. A.M., and its reappearance at 3h. 53m. 52s. A.M. It is stated by Webb that only four observations of the disappearance and reappearance of the second satellite on the same night are on record. A transit egress of the second satellite at 0h. 52m. A.M. on the 26th. On the 29th a transit ingress of the shadow of the first satellite at 2h. 35m. A.M.; a transit ingress of the satellite at 3h. 58m. A.M.; an eclipse disappearance of the first satellite at 11h. 45m. 28s. P.M. On the 30th an occultation reappearance of the first satellite at 3h. 17m. A.M. On the 30th a transit egress of the shadow of the first satellite at 11h. 16m. P.M. On the 31st a transit egress of the first satellite at 0h. 37m. A.M. The following are the times of superior and inferior conjunctions of the fourth satellite :—Superior, August 16th, 3h. 9m. P.M.; Inferior, August 8th, 7h. 12m. A.M., 25th, 1h. 38m. A.M.

Saturn is still just visible in the evening, but must be looked for in the early twilight to be seen at all. He sets

on the 1st at 9h. 50m. P.M., or 2h. 4m. after sunset, with a southern declination of 1° 10′, and an apparent equatorial diameter of 16″ (the major axis of the ring system being 37¼″ in diameter, and the minor 4·7″). On the 31st he sets at 7h. 56m. P.M., or 1h. 8m. after sunset, with a southern declination of 2° 22′, and an apparent equatorial diameter of 15½″ (the major axis of the ring system being 36″ in diameter, and the minor 5″). Iapetus is at his greatest eastern elongation on the 11th, and in inferior conjunction on the 30th. A map of the path of Saturn during August will be found in the "Face of the Sky" for March. He is in conjunction with the beautiful pair γ Virginis shortly after 10h. A.M. on the 7th, the planet being 31′ south of the star.

The above remarks as to the visibility of Saturn apply also to Uranus. He sets on the 1st at 10h. 30m. P.M., with a southern declination of 13° 23′, and an apparent diameter of 3·6″. On the 31st he sets at 8h. 33m. P.M., with a southern declination of 13° 40′. A map of his path during August will be found in the "Face of the Sky" for April.

This month is one of the most favourable ones for the observation of shooting stars. The most noted shower is that of the Perseids with a radiant point at the maximum display on August 10th in R.A., 2h. 52m., dec. + 56°. Observation of this region of the heavens with an opera glass will no doubt show stationary meteors, or meteors which shift their positions very slowly. Their place, and the direction of their shift, should be noted for the purpose of determining whether the radiant is a geometrical point, or a circle, or an elliptic area, as suggested with regard to the November meteors. (*Monthly Notices of the R.A.S.*, Vol. xlvii., pp. 69-73.) The radiant point souths on the 10th at 5h. 37m. A.M.

The Moon enters her last quarter at 4h. 23m. P.M. on the 5th; is new at 8h. 48m. P.M. on the 11th; enters her first quarter at 9h. 52m. A.M. on the 19th, and is full at 8h. 43m. A.M. on the 27th. She is in perigee at 9h. P.M. on the 8th (distance from the earth 227,130 miles), and in apogee at 7h. P.M. on the 20th (distance from the earth 251,150 miles).

Chess Column.

By C. D. Locock, B.A.Oxon.

ALL COMMUNICATIONS for this column should be addressed to the "CHESS EDITOR, *Knowledge Office*," and posted before the 10th of each month.

Solution of July Problem (G. K. Ansell) :—

Key-move, 1. R to B5.

If 1. . . . K × R, 2. P to K4ch.
If 1. . . . P × R, 2. B × Pch.
If 1. . . . Anything else, 2. R × Pch.

CORRECT SOLUTIONS received from R. B. Cooke, Alpha.

The defence to 1. Kt (Q5) to Kt6 (or B3, or K3), in Mr. Loyd's problem in the June number, is 1. . . . P to Kt8, becoming a Bishop. If then 2. . . . B to Q5 Black is stale-mated. R. I. has taken the hint and discovered this.

In Mr. Donisthorpe's problem the Bishop must work his way to KKt7, but he must capture the Black Pawn *en route* should Black play 1. . . . B to R8, for otherwise Black will secure a stale-mate by 2. . . . P to Kt7. The only key, therefore, is 1. B to K5.

This problem is correctly solved by R. I. and Alpha.

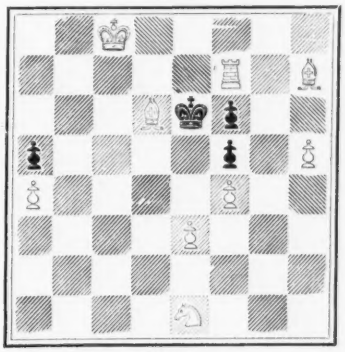
W. James.—If 1. Kt (B8) to K7, Black defends himself by checking with the Rook, either at once or on the second move.

Alpha.—The mysterious defence is given above. Many thanks for the position appended.

PROBLEM.

By A. G. FELLOWS.

BLACK.



WHITE.

White to play and mate in three moves.

The following position from a Sanscrit work (translated in 1818) is contributed by "Alpha." The solution is more showy than difficult :—

White.—K at KB8, Q at QR2, R at KKt7, Kt at KB6, P at KKt5.

Black.—K at KRsq, B at QKt8. White to play and mate with the Pawn in four moves.

The late Mr. Rosenbaum once composed a somewhat neater and certainly more difficult problem, with precisely the same conditions :—

White.—K at KR6, R at K7, Kt at QB7, P at K4.

Black.—K at KRsq. White mates with the Pawn in four moves.

The following game was played on the Atlantic last month, the players being A. Walbrodt, on his way to Berlin, and Max Judd, the recently appointed U.S. Consul at Vienna. The score is from the *Illustrated London News* :—

"FRENCH DEFENCE."

WHITE (Max Judd).	BLACK (A. Walbrodt).
1. P to K4	1. P to K3
2. P to Q4	2. P to Q4
3. QKt to B3	3. KKt to B3
4. B to KKt5	4. B to K2
5. P to K5	5. KKt to Q2
6. B × B	6. Q × B
7. Q to Kt4 (*a*)	7. P to KKt3 ?
8. P to KB4	8. P to QR3 (*b*)
9. Castles	9. P to QB4
10. P × P	10. Kt × P ? (*c*)
11. Kt to B3 ? ? (*d*)	11. Kt to B3
12. P to KR4 (*e*)	12. P to KR4
13. Q to Kt5	13. P to QKt4
14. B to Q3	14. B to Kt2
15. Q × Q	15. K × Q
16. Kt to Kt5 ?	16. KR to Qsq
17. KR to Ksq	17. Kt to Kt5
18. P to B3	18. QKt × B
19. P × Kt	19. QR to Bsq
20. P to Q4	20. Kt to R5
21. R to Q3	21. P to R4

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22. K to Ktsq	22. Kt to Kt3 (*f*)
23. Kt × KtP	23. B to R3
24. R to Kt3	24. Kt to B5
25. Kt to KB3 (*g*)	25. R to QKtsq
26. P to R4	26. Kt to Kt3
27. R to R3 (*h*)	27. B × Kt (*i*)
28. P × B	28. Kt to B5
29. R to Kt3	29. R to Kt2
30. K to R2 (*j*)	30. KR to QKtsq
31. R to K2	31. P to R5 ! (*k*)
32. R to Kt4	32. R × P
33. R × R	33. R × R
34. R to QB2	34. R to Kt6 !
35. Kt to Q2 (*l*)	35. Kt × Kt
36. R × Kt	36. R to Kt6
37. K to Ktsq (*m*)	37. R to Kt5
38. R to KB2	38. R × RP
39. P to KKt3 (*n*)	39. R to R8ch
40. K to B2	40. R to R8
41. K to B3 ? (*o*)	41. P to R6
42. P to QKt4 (*p*)	42. R to KKt8
43. P to Kt5	43. R × Pch
44. K to Kt4	44. K to Q2
45. R to B2	45. R to Kt8
46. K to R5 ?	46. R to Kt8
47. P to Kt6	47. R to Kt7
48. R to B3	48. P to R7
49. R to QR3	49. P to R5
50. K to R6	50. P to R6 and wins.

NOTES.

(*a*) Inferior to the usual 7. Q to Q2, P to QR3 ; 8. Kt to Qsq, P to QB4 ; 9. P to QB3, because it leaves the Queen's side weak. Black's reply, however, is feeble, castling being quite safe.

(*b*) The customary preparation for P to QB4 in this opening. White's reply is perhaps a little premature.

(*c*) A strange oversight, followed by a still more strange omission on the part of White. He should of course play 10. . . QKt to B3 at once, as the Pawn will keep. Possibly the moves were incorrectly transposed in the score.

(*d*) 11. Kt × P obviously wins at once, whether the Knight be taken or not.

(*e*) The weakness of Pawns caused by this advance ultimately loses him the game (*vide* Black's 36th and 37th moves) : but he has already some inferiority.

(*f*) A very enterprising line of play : the Pawn sacrificed is not regained for several moves.

(*g*) Showing the futility of his 16th move.

(*h*) 27. Kt to B3 would not save the Pawn—*e.g.*, 27. Kt to B3, Kt × P ; 28. R × R, Kt × Ktch, etc.

(*i*) 27. . . Kt × P might also be played, the Bishop in such a position being worth more than the Knight.

(*j*) More desirable would be 30. R. to K2 at once, with a view to manœuvring the Knight *viâ* Ksq to Q3, etc.

(*k*) A fine move, which leaves White in a cramped position after the exchanges.

(*l*) This soon loses a Pawn ; 35. K to Rsq, with a view to R to B3 is certainly no worse.

(*m*) A strange position. White is compelled to make some disadvantageous move : for if 37. R to KB2, R to Q6 wins.

(*n*) 39. K to R2, R to R8 ; 40. K to R3, R to R8ch ; 41. K to Kt4 looks more promising.

(*o*) Even now 41. R to B3 might save the game.

(*p*) For if 42. R to B3, P to R7 ! ; 43, R × P, R × Pch ; changing Rooks and winning with the RP. The ending is very cleverly played by Herr Walbrodt.

CHESS INTELLIGENCE.

"KNOWLEDGE" THREE-MOVE PROBLEM TOURNAMENT.

This will commence in the November number of KNOWLEDGE, and is open to the world.

The Proprietors of KNOWLEDGE offer the following prizes :—

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The Conditions are as follows :—

1. Each competitor may send not more than one three-move unconditional direct-mate problem (diagrammed).

2. Competing positions must be original and unpublished.

3. Each problem must be accompanied by a motto and full solution, with a sealed envelope containing the composer's name and address.

4. Competing positions must reach Mr. C. D. Locock, Burwash, Sussex, England, on or before October 10th, 1893.

5. The Chess Editor reserves the right of excluding manifestly impossible, unsound, or inferior positions.

6. Should more than twenty positions be received, the Chess Editor may, with the assistance of an expert, select the best twenty for competition. In that case the remainder will be returned to their respective composers without delay.

7. The adjudication will be partly by solvers and partly by a recognized expert.

All solvers who solve correctly every problem will be entitled to vote on their merits, the Chess Editor having also one vote. The six or seven problems thus selected will then be submitted to an expert, whose decision on their respective merits will be final.

A SOLUTION TOURNEY will commence at the same time. Particulars will be given next month.

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